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THE INSIDE HISTORY OF A GREAT MEDICAL DISCOVERY

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THE construction of the Panama Canal was made possible because it was shown that yellow fever, like malaria, could be spread only by the bites of infected mosquitoes.

The same discovery, which has been repeatedly referred to as the greatest medical achievement of the twentieth century, was the means of stamping out the dreaded scourge in Cuba, as well as in New Orleans, Rio de Janeiro, Vera Cruz, Colon, Panama and other cities in America.

This article is intended to narrate the motives that led up to the investigation and also the manner in which the work was planned, executed and terminated. No names are withheld and the date of every important event is given, so that an interested reader may be enabled to follow closely upon the order of things as they occurred and thus form a correct idea of the importance of the undertaking, the risk entailed in its accomplishment and how evenly divided was the work among those who, in the faithful performance of their military duties, contributed so much for the benefit of mankind; the magnitude of their achievement is of such proportions, that it loses nothing of its greatness when we tear away the halo of apparent heroism that well-meaning but ignorant historians have thrown about some of the investigators.

The whole series of events, tragic, pathetic, comical and otherwise, took place upon a stage made particularly fit by nature and the surrounding circumstances.

Columbia Barracks, a military reservation, garrisoned by some fourteen hundred troops, distant about eight miles from the city of Havana, the latter, suffering at the time from an epidemic of yellow fever, which the application of all sanitary measures had failed to check or ameliorate and finally, our experimental camp (Camp Lazear), a few army tents, securely hidden from the road leading to Marianao, and safeguarded against intercourse with the outside world; the whole setting portentously silent and gloriously bright in the glow of tropical sunlight and the green of luxuriant vegetation.

Two members of a detachment of four medical officers of the United States Army, on the morning of August 31, 1900, were busily examining under microscopes several glass slides containing blood from a fellow officer who, since the day before, had shown symptoms of yellow fever; these men were Drs. Jesse W. Lazear and myself; our sick colleague was Dr. James Carroll, who presumably had been infected by one of our "experiment mosquitoes."

It is very difficult to describe the feelings which assailed us at that moment; a sense of exultation at our apparent success no doubt animated us; regret, because the results had evidently brought a dangerous illness upon our coworker and with it all associated a thrill of uncertainty for the reason of the yet insufficient testimony tending to prove the far-reaching truth which we then hardly dared to realize.

As the idea that Carroll's fever must have been caused by the mosquito that was applied to him four days before became fixed upon our minds, we decided to test it upon the first non-immune person who should offer himself to be bitten; this was of common occurrence and taken much as a joke among the soldiers about the military hospital. Barely fifteen minutes may have elapsed since we had come to this decision when, as Lazear stood at the door of the laboratory trying to "coax" a mosquito to pass from one test-tube into another, a soldier came walking by towards the hospital buildings; he saluted, as it is customary in the army upon meeting an officer, but, as Lazear had both hands engaged, he answered with a rather pleasant "Good morning." The man stopped upon coming abreast, curious no doubt to see the performance with the tubes, and after gazing for a minute or two at the insects he said: "You still fooling with mosquitoes, Doctor?" "Yes," returned Lazear, "will you take a bite?" "Sure I ain't scared of 'em," responded the man. When I heard this, I left the microscope and stepped to the door, where the short conversation had taken place; Lazear looked at me as though in consultation; I nodded assent, then turned to the soldier and asked him to come inside and bare his forearm. Upon a slip of paper I wrote his name while several mosquitoes took their fill; William H. Dean, American by birth, belonging to Troop B, Seventh Cavalry; he said that he had never been in the tropics before and had not left the military reservation for nearly two months. The conditions for a test case were quite ideal.

I must say we were in great trepidation at the time; and well might we have been, for Dean's was the first indubitable case of yellow fever about to be produced experimentally by the bite of purposely infected mosquitoes. Five days afterwards, when he came down with yellow fever and the diagnosis of his case was corroborated by Dr. Roger P. Ames, U. S. Army, then on duty at the hospital, we sent a cablegram to Major Walter Reed, chairman of the board, who a month before had

been called to Washington upon another duty, apprising him of the fact that the theory of the transmission of yellow fever by mosquitoes, which at first was doubted so much and the transcendental importance of which we could then barely appreciate, had indeed been confirmed.

State of Things before the Discovery of Mosquito Transmission

Other infectious diseases, tuberculosis, for instance, may cause a greater death-rate and bring about more misery and distress, even to-day, than yellow fever has produced at any one time; but no disease, except possibly cholera or the plague, is so tragic in its development, so appalling in its action, so devastating in its results, nor does any other make greater havoc than yellow fever when it invades non-immune or susceptible communities.

For two centuries, at least, the disease has been known to exist endemically, that is, more or less continuously, in most of the Mexican Gulf ports, extending its ravages along the West India Islands and the cities of the Central and the South American coast.

In the United States it has made its appearance in epidemic form as far north as Portsmouth, N. H. At Philadelphia in 1793, more than ten per cent. of the entire population died of yellow fever. Other cities, like Charleston, S. C., suffered more than twenty epidemics in as many summers, during the eighteenth century. In the city of New Orleans, the epidemic which developed in the summer of 1853 caused more than 7,000 deaths. Later, in 1878, yellow fever invaded 132 towns in the United States, producing a loss of 15,932 lives out of a total number of cases which reached to more than 74,000: New Orleans alone suffered a mortality of 4,600 at that time. Recently (1905), this city withstood what is to be hoped shall prove its last invasion, which, thanks to the modern methods employed in its suppression, based upon the new mosquito doctrine, only destroyed about 3,000 lives.

It is by contemplating this awful record, and much more there is which for the sake of brevity I leave unstated, that one realizes the boon to mankind which the successful researches of the Army Board have proved. The work of prevention, the only one that may be considered effective when dealing with the epidemic diseases, was entirely misguided with regard to yellow fever until 1901: the sick were surrounded by precautions which were believed most useful in other infectious diseases, the attendants were often looked upon as pestilential, and so treated, in spite of the fact that evidence from the early history of the disease clearly pointed to the apparent harmlessness even of the patients themselves. All this notwithstanding, cases continued to develop, in the face of shotgun quarantine even, until the last non-immune inhabitant of the locality had been either cured or buried.

The mystery which accompanied the usual course of an epidemic, the

poison creeping from house to house, along one side of a street, seldom, crossing the road, spreading sometimes around the whole block of houses before appearing in another neighborhood, unless distinctly carried there by a visitor to the infected zone who himself became stricken, all this series of peculiar circumstances was a never-ending source of discussion and investigation.

In the year 1900, Surgeon H. R. Carter, of the then Marine Hospital Service, published a very interesting paper calling attention to the interval of time which regularly occurred between the first case of yellow fever in a given community and those that subsequently followed; this was never less than two weeks, a period of incubation extending beyond that usually accorded to other acute infectious diseases. The accuracy of these observations has later been confirmed by the mosquito experiments hereinafter outlined.

Factors which led to the Appointment of the Board

One may well believe that such a scourge as yellow fever could not have been long neglected by medical investigators, and so we find that from the earliest days, when the germ-theory of disease took its proper place in modern science, a search for the causative agent of this infection was more or less actively instituted.

Men of the highest attainments in bacteriology engaged in numerous attempts to isolate the yellow fever microbe: unfortunately not a few charlatans took advantage of the dread and terror which the disease inspires, to proclaim their discoveries and their specific *cures*; one of these obtained wealth and honor in one of the South American republics for presumably having discovered the "germ" and prepared a so-called vaccination which was expected to eradicate the disease from that country, but for many years after the foreign population continued to suffer as before and the intensity and the spread of yellow fever remained unabated, although thousands of "preventive inoculations" were made every month.

Geo. M. Sternberg in 1880, then an army surgeon, was directly instrumental in exposing the swindle that was being perpetrated, putting an end, after the most painstaking investigation, to all the claims to discovery of the "germ" of yellow fever that had been made by several medical men in Spanish America. The experience which he obtained during a scientific excursion through Mexico, Cuba and South America gave him a wonderful insight as to the difficulties one has to contend with in such work and made him realize the importance of special laboratory training for such undertaking. It is interesting to note that, as surgeon general of the U. S. Army, twenty years after, General Sternberg chose and appointed the men who constituted the yellow fever board, in Cuba.

The year before the Spanish-American war, an Italian savant, who had obtained a well-deserved reputation as bacteriologist while working in the Institute Pasteur of Paris, came out with the announcement from Montevideo, Uruguay, that he had actually discovered the much-sought-for cause of yellow fever; his descriptions of the methods employed, though not materially different from those followed by Sternberg many years before, bore the imprint of truth and his experimental inoculations had apparently been successful. Sanarelli—that is his name—for about two years was the "hero of the hour," yet his claims have been proved absolutely false.

The question of the identity of his "germ" was first taken up by the writer under instructions from General Sternberg: during the Santiago campaign I had opportunity to autopsy a considerable number of yellow-fever cases and, following closely upon Sanarelli's directions, only three times out of ten could his bacillus be demonstrated; at almost the same time, Drs. Reed and Carroll, in Washington, were carrying out experiments which showed that Sanarelli's bacillus belonged to the hog-cholera group of bacteria and thus when found in yellow fever cadavers could play there only a secondary rôle as far as the infection is concerned.

Unfortunately, two investigators belonging to the U. S. Marine Hospital Service, Drs. Wasdin and Geddings, were, according to their claims, corroborating Sanarelli's findings: there was nothing to do but that the investigation should continue, and so I was sent by General Sternberg to Havana in December, 1898, with instructions and power to do all that might be necessary to clear up the matter. Wasdin and Geddings had preceded me; the work carried us through the summer of 1899; we frequently investigated the same cases; I often autopsied bodies from which we took the same specimens and made the same cultures, in generally the same kind of media, and finally we rendered our reports to our respective departments, Wasdin and Geddings affirming that Sanarelli's bacillus was present in almost all the cases, while I denied that it had such specific character and showed its occurrence in cases not yellow fever. A virulent epidemic which raged in the city of Santiago and vicinity during 1899 afforded me abundant material for research.

In the meantime the city of Havana was being rendered sanitary in a way which experience had taught would have overcome any bacterial infection, and, in fact, the diseases of filth, such as dysentery, tuberculosis, children's complaints and others, decreased in a surprising manner, while yellow fever seemed to have been little affected if at all.

Evidently, a more thorough overhauling of the matter was necessary to arrive at the truth, and while the question of Sanarelli and his claims was practically put aside, Surgeon-General Sternberg, recognizing the importance of the work before us and that its proportions were such as to render the outcome more satisfactory by the cooperation of several

investigators in the same direction, wisely decided to create a board for the purpose and so caused the following to be issued:

Special Orders }
No. 122 }

HEADQUARTERS OF THE ARMY,
ADJUTANT GENERAL'S OFFICE,
WASHINGTON, May 24, 1900

Extract

34. By direction of the Secretary of War, a board of medical officers is appointed to meet at Camp Columbia, Quemados, Cuba, for the purpose of pursuing scientific investigations with reference to the infectious diseases prevalent on the Island of Cuba. Detail for the board:

Major Walter Reed, surgeon, U. S. Army;
Acting Assistant Surgeon James Carroll, U. S. Army;
Acting Assistant Surgeon Aristides Agramonte, U. S. Army;
Acting Assistant Surgeon Jesse W. Lazear, U. S. Army.

The board will act under general instructions to be communicated to Major Reed by the Surgeon General of the Army.

By command of MAJOR GENERAL MILES,
H. C. CORBIN,
Adjutant General

It may be of interest to the reader to learn who these men were and the reasons why they were probably selected for the work.

Major Reed, the first member in the order of appointment, was the ranking officer and therefore the chairman of the board. He was a regular army officer, at the time curator of the Army Medical Museum in Washington and a bacteriologist of some repute. He deservedly enjoyed the full confidence of the surgeon general, besides his personal friendship and regard. Reed was a man of charming personality, honest and above board. Every one who knew him loved him and confided in him. A polished gentleman and a scientist of the highest order, he was peculiarly fitted for the work before him.

Dr. James Carroll, the second member of the board, was a self-made man, having risen from the ranks through his own efforts: while a member of the Army Hospital Corps he studied medicine and subsequently took several courses at Johns Hopkins University in the laboratory branches. At the time of his appointment to the board he had been for several years an able assistant to Major Reed. Personally, Carroll was industrious and of a retiring disposition.

Dr. Jesse W. Lazear was the fourth member of the board. He had graduated from the College of Physicians and Surgeons (Columbia University) in the same class as the writer, in 1892, and had afterwards studied abroad and at Johns Hopkins. Lazear had received special training in the investigation of mosquitoes with reference to malaria and other diseases. Stationed at Columbia Barracks, he had been in

Cuba several months before the board was convened, in charge of the hospital laboratory at the camp. A thorough university man, he was the type of the old southern gentleman, kind, affectionate, dignified, with a high sense of honor, a staunch friend and a faithful soldier.

The writer was the third member of the Army Board. Born in Cuba during the ten years' war, while still a child, my father having been killed in battle against the Spanish, I was taken to the United States and educated in the public schools and in the College of the City of New York, graduating from the College of Physicians and Surgeons in 1892. At the breaking out of the war I was assistant bacteriologist in the New York Health Department. The subject of yellow fever research was my chief object from the outset, and, at the time the board was appointed, I was in charge of the laboratory of the Division of Cuba, in Havana.

It may be readily seen from the brief sketch regarding the several members that the components of the yellow-fever board really constituted a perfectly consistent body, for the reason, mainly, that they were all men trained in the special field wherein their labors were to be so fruitful and that before their appointment to the board they had been more or less associated in scientific work.

First Part of the Work of the Board

My first knowledge of the existence of the board was had through the following letter from my friend Major Reed:

War Department,
Surgeon General's Office,

Washington, May 25, 1900

Dr. A. Agramonte,
Act'g Asst. Surgeon U. S. A.,
Military Hospital No. 1,
Havana, Cuba

My dear Doctor: An order issued yesterday from the War Department calls for a board of medical officers for the investigation of acute infectious diseases occurring on the Island of Cuba. The board consists of Carroll, yourself, Lazear and the writer. It will be our duty, under verbal instructions from the Surgeon General, to continue the investigation of the causation of yellow fever. The Surgeon General expects us to make use of your laboratory at Military Hospital No. 1 and Lazear's laboratory at Camp Columbia.

According to the present plan, Carroll and I will be quartered at Camp Columbia. We propose to bring with us our microscopes and such other apparatus as may be necessary for the bacteriological and pathological work. If, therefore, you will promptly send me a list of the apparatus on hand in your laboratory, it will serve as a very great help in enabling us to decide as to what we should include in our equipment. Any suggestions that you may have to make will be much appreciated.

Carroll and I expect to leave New York, on transport, between the 15th and 20th of June and are looking forward, with much pleasure, to our association

with you and Lazear in this interesting work. As far as I can see we have a year or two of work before us.

Trusting you will let me hear from you promptly, and with best wishes,

Sincerely yours,

(Signed) WALTER REED

On the afternoon of June 25, 1900, the four officers met for the first time in their new capacity, on the veranda of the officers' quarters at Columbia Barracks Hospital. We were fully appreciative of the trust and aware of the responsibility placed upon us and with a feeling akin to reverence heard the instructions which Major Reed had brought from the surgeon general; they comprised the investigation also of malaria, leprosy and unclassified febrile conditions, and were given with such detail and precision as only a man of General Sternberg's experience and knowledge in such matters could have prepared. After deciding upon the first steps to be taken, it was unanimously agreed that whatever the result of our investigation should turn out to be, it was to be considered as the work of the board as a body, and never as the outcome of any individual effort; that each one of us was to work in harmony with a general plan, though at liberty to carry out his individual methods of research. We were to meet whenever necessary, Drs. Reed, Carroll and Lazear to remain at the Barracks Hospital and I to stay in charge of the laboratory in Havana, at the Military Hospital, where I also had a ward into which yellow-fever cases from the city were often admitted.

Work was begun at once. Fortunately for our purpose, an epidemic of yellow fever existed in the town of Quemados, in close proximity to the military reservation of Camp Columbia. Even before the arrival of Reed and Carroll, Lazear and I had been studying its spread, following the cases very closely; subsequently a few autopsies were made by me, Carroll making cultures from the various tissues and Lazear securing fragments for microscopical examination; a careful record was kept and the results noted; cases gradually became less in number as the epidemic slowly died out, about the middle of August.

In the meantime a rather severe outbreak of yellow fever had occurred in Santa Clara, a city in the interior of the island, having invaded the garrison and caused the death of several soldiers; as the origin of the infection was shrouded in mystery, and cases continued to appear among the troops even after they had moved out of the town, it was agreed that I should endeavor to trace the source of the epidemic and aid the medical authorities in establishing whatever preventive measures might seem proper. This service is here recorded because in the general discussion of the start and course of the epidemic with Dr. J. Hamilton Stone, the officer in charge of the military hospital, we incidentally spoke of the possible agency of insects in spreading the disease, pointing particularly in this direction the fact of the infection of a trooper who,

suffering from another complaint, occupied a bed in a ward across the yard from where a yellow fever case had developed two weeks before.

The infection of the city of Santa Clara had evidently taken place from Havana, distant only one night's journey by train. Captain Stone, a particularly able officer, had already instituted effective quarantine measures before my arrival, so that I only remained there a few days.

But as to the actual cause of the disease we were still entirely at sea; it helped us little to know that a man could be infected in Havana, take the train for a town in the interior and start an outbreak there in the course of time.

Upon rejoining my colleagues (July 2) we resumed our routine investigations; not only in Quemados, where the disease was being stamped out, but also in Havana, at "Las Animas" Hospital and at Military Hospital No. 1, where my laboratory (the division laboratory) was located. There was no scarcity of material and the two members who until then had never seen a case of yellow fever (Reed and Carroll) had ample opportunity, and took advantage of it, to become acquainted with the many details of its clinical picture which escape the ordinary practitioner, the knowledge and the appreciation of which, in their relative value, give the right to the title of "expert."

Since the later part of June, reports had been coming to headquarters of an extraordinary increase of sickness among the soldiers stationed at Pinar del Rio, the capital of the extreme western province, and very soon the great mortality from so-called "pernicious malarial fever" attracted the attention of the chief surgeon, Captain A. N. Stark, who, after consulting with Major Reed, ordered me to go there and investigate. A man had died, supposedly from malaria, just before my arrival on the afternoon of July 19. The autopsy which I performed at once showed me that yellow fever had been the cause of his death, and a search through the military hospital wards revealed the existence of several unrecognized cases being treated as malaria; a consultation held with the medical officer in charge showed me his absolute incapacity, as he was under the influence of opium most of the time (he committed suicide several months afterwards), and so I telegraphed the condition of things to headquarters; in answer I received the following:

CHIEF SURGEON'S OFFICE,
HDQRS. DEPT. HAVANA AND PINAR DEL RIO,
QUEMADOS, CUBA, July 20, 1900

SURGEON AGRAMONTE,
Pinar del Rio Barracks,
Pinar del Rio, Cuba

Report received last night. My thanks are due for your prompt action and confirmation of my suspicions.

STARK,
Chief Surgeon

Conditions in the hospital were such as to demand immediate action; the commander of the post refused to believe he had yellow fever among his 900 men and was loath to abandon his comfortable quarters for the tent life in the woods that I earnestly recommended. In answer to my telegram asking for official support, I received the following:

CHIEF SURGEON'S OFFICE,
HDQRS. DEPT. HAVANA AND PINAR DEL RIO,
QUEMADOS, CUBA, July 21, 1900

SURGEON AGRAMONTE,
Pinar del Rio Barracks,
Pinar del Rio, Cuba

Take charge of cases. Reed goes on morning train. Wire for anything wanted. Nurses will be sent. Instructions wired commanding officer. Other doctors should not attend cases. Establish strict quarantine at hospital. You will be relieved as soon as an immune can be sent to replace you. Report daily by wire.

STARK,
Chief Surgeon

When Major Reed came to Pinar del Rio (July 21) I had, the day before, established a separate yellow-fever hospital, under tents, attended by some of the men who had already passed an attack and were thus immune. The Major and I went over the ground very carefully, we studied the sick report for two months back, fruitlessly trying to place the blame upon the first case. I well remember how, as we stood in the men's sleeping quarters, surrounded by a hundred beds, from several of which fatal cases had been removed, we were struck by the fact that the later occupants had not developed the disease. In connection with this, and particularly interesting, was the case of a soldier prisoner who had been confined to the guard-house since June 6; he showed the first symptoms of yellow fever on the twelfth and died on the eighteenth; none of the other eight prisoners in the same cell caught the infection, though one of them continued to sleep in the same bunk previously occupied by his dead comrade. More than this; the three men who handled the clothing and washed the linen of those who had died during the last month were still in perfect health. Here we seemed to be in the presence of the same phenomenon remarked by Captain Stone in reference to his case at Santa Clara, and before that by several investigators of yellow-fever epidemics; the infection at a distance, the harmless condition of bedding and clothing of the sick; the possibility that some insect might be concerned in spreading the disease deeply impressed us and Major Reed mentions the circumstance in his later writings. This was really the first time that the mosquito transmission theory was seriously considered by members of the board, and it was decided that, although discredited by the repeated failure of its most ardent supporter, Dr. Carlos J. Finlay, of Havana, to demonstrate it, the matter should be taken up by the board and thoroughly sifted.

The removal of the troops out of Pinar del Rio was the means of at once checking the propagation of the disease.

On the first day of August the board met and after due deliberation determined to investigate mosquitoes in connection with the spread of yellow fever. As Dr. Lazear was the only one of us who had had any experience in mosquito work, Major Reed thought proper that he should take charge of this part of the investigation in the beginning, while we, Carroll and I, continued with the other work on hand, at the same time gradually becoming familiar with the manipulations necessary in dealing with the insects.

A visit was now made to Dr. Finlay, who, much elated at the news that the board was about to investigate his pet theory, the transmission of yellow fever from man to man by mosquitoes, very kindly explained to us many points regarding the life of the one kind he thought most guilty and ended by furnishing us with a number of eggs which, laid by a female mosquito nearly a month before, had remained unhatched on the inside of a half empty bowl of water in his library.

Much to our disappointment and regret, during the first week of August, Major Reed was recalled to Washington that he might, in collaboration with Drs. Vaughan and Shakespeare, complete the report upon "Typhoid Fever in the Army." Thus we were deprived of his able counsel during the first part of the mosquito research. Major Reed was detained longer than he expected and could not return to Cuba until early in October, several days after Lazear's death.

The mosquito eggs obtained from Dr. Finlay hatched out in due time; the insects sent to Washington for their exact classification were declared by Dr. L. O. Howard, entomologist to the Agricultural Department, to be *Culex fasciatus.* Later, they have been called *Stegomyia fasciatus* and now go under the name of *Stegomyia calopus* (*Aedes cal.*).

Lazear applied some of these mosquitoes to cases of yellow fever at "Las Animas" Hospital, keeping them in separate glass tubes properly labeled, and every thing connected with their bitings was carefully recorded; the original batch soon died and the work was carried on with subsequent generations from the same.

The lack of material at Quemados caused us to remove our field of action to Havana, where cases of yellow fever continued to appear. We met almost every day at "Las Animas" Hospital, where Lazear was trying to infect his mosquitoes, or now and then I performed autopsy upon a case, and Carroll secured sufficient cultures to last him for several days of bacteriological investigation.

Considering that, in case our surmise as to the insect's action should prove to be correct, it was dangerous to introduce infected mosquitoes amongst a population of 1,400 non-immunes at Camp Columbia, Dr. Lazear thought best to keep his presumably infected insects in my labo-

ratory at the Military Hospital No. 1, from where he carried them back and forth to the patients who were periodically bitten.

Incidentally, after the mosquitoes fed upon the yellow fever patients, they were applied, at intervals of two or three days, to whoever would consent to run the risk of contracting yellow fever in this way; needless to say, current opinion was against this probability and as time passed and numerous individuals who had been bitten by insects which had previously fed upon yellow fever blood remained unaffected, I must confess that even the members of the board, who were rather sanguine in their expectations, became somewhat discouraged and their faith in success very much shaken.

No secret was made of our attempts to infect mosquitoes; in fact many local physicians became intensely interested, and Lazear and his tubes were the subject of much comment on the part of the Havana doctors, who nearly twenty years before had watched and laughed at Dr. Finlay, then bent apparently upon the same quest in which we were now engaged. Dr. Finlay himself was somewhat chagrined when he learned of our failure to infect any one with mosquitoes, but, like a true believer, was inclined to attribute this negative result more to some defect in our technique than to any flaw in his favorite theory.

Although the board had thought proper to run the same risks, if any, as those who willingly and knowingly subjected themselves to the bites of the supposedly infected insects, opportunity did not offer itself readily, since Major Reed was away in Washington and Carroll, at Camp Columbia, engrossed in his bacteriological investigations came to Havana only when an autopsy was on hand or a particularly interesting case came up for study. I was considered an immune, a fact that I would not like to have tested, for though born in the island of Cuba, I had practically lived all my life away from a yellow-fever zone; it was therefore presumed that I ran no risk in allowing mosquitoes to bite me, as I frequently did, just to feed them blood, whether they had previously sucked from yellow-fever cases or not. And so, time passed and several Americans and Spaniards had subjected themselves in a sporting mood to be bitten by the infected (?) mosquitoes without causing any untoward results, when Lazear applied to himself (August 16, 1900) a mosquito which ten days before had fed upon a mild case of yellow fever in the fifth day of his disease; the fact that no infection resulted, for Lazear continued in excellent health for a space of time far beyond the usual period of incubation, served to discredit the mosquito theory in the opinion of the investigators to a degree almost beyond redemption, and the most enthusiastic, Dr. Lazear himself, was almost ready to "throw up the sponge."

I had as laboratory attendant a young American, a private belonging to the Hospital Corps of the Army, who more than once had bared his

arm to allow a weak mosquito a fair meal with which to regain its apparently waning strength; Loud, for that was his name, derided the idea that such a little beast could do so much harm as we seemed ready to accuse it of, although he was familiar with the destruction caused by bacteria, but then, he used to say, "bacterias work in armies of more than a million bugs at the same time and no one would be d—— fool enough to let more than one or two gnats sting him at once."

This state of things, the gradual loss of faith in the danger which mosquitoes seemed to possess, led Dr. Lazear to relax a little and become less scrupulous in his care of the insects, and often, after applying them to patients, if pressed for time, he would take them away with him to his laboratory at Columbia Barracks, where, the season being then quite warm, they could be kept as comfortably as at the Military Hospital laboratory. Thus it happened that on the twenty-seventh of August he had spent the whole morning at "Las Animas" Hospital getting his mosquitoes to take yellow-fever blood: the procedure was very simple; each insect was contained in a glass tube covered by a wad of cotton, the same as is done with bacterial cultures. As the mouth of the tube is turned downwards, the insect usually flies towards the bottom of the tube (upwards), then the latter is uncovered rapidly and the open mouth placed upon the forearm or the abdomen of the patient; after a few moments the mosquito drops upon the skin and if hungry will immediately start operations; when full, by gently shaking the tube, the insect is made to fly upwards again and the cotton plug replaced without difficulty. It so happened that this rather tedious work, on the day above mentioned, lasted until nearly the noon hour, so that Lazear, instead of leaving the tubes at the Military Hospital, took them all with him to Camp Columbia: among them was one insect that for some reason or other had failed to take blood when offered to it at "Las Animas" Hospital.

This mosquito had been hatched in the laboratory and in due time fed upon yellow-fever blood from a severe case on August 15, that is, twelve days before, the patient then being in the second day of his illness; also at three other times, six days, four days and two days before. Of course, at the time, no particular attention had been drawn to this insect, except that it refused to suck blood when tempted that morning.

After luncheon that day, as Carroll and Lazear were in the laboratory attending to their respective work, the conversation turning upon the mosquitoes and their apparent harmlessness, Lazear remarked how one of them had failed to take blood, at which Carroll thought that he might try to feed it, as otherwise it was liable to die before next day (the insect seemed weak and tired); the tube was carefully held first by Lazear and then by Carroll himself, for a considerable length of time, upon his forearm, before the mosquito decided to introduce its proboscis.

This insect was again fed from a yellow fever case at "Las Animas"

Hospital on the twenty-ninth, two days later, Dr. Carroll being present, though not feeling very well, as it was afterwards ascertained.

We three left the yellow-fever hospital together that afternoon; I got down from the doherty-wagon where the road forks, going on to the Military Hospital, while Carroll and Lazear continued on their way to Camp Columbia. On the following day, Lazear telephoned to me in the evening, to say that Carroll was down with a chill after a sea bath taken at the beach, a mile and a half from Camp, and that they suspected he had malaria; we therefore made an appointment to examine his blood together the following morning.

When I reached Camp Columbia I found that Carroll had been examining his own blood early that morning, not finding any malarial parasites; he told me he thought he had "caught cold" at the beach: his suffused face, blood-shot eyes and general appearance, in spite of his efforts at gaiety and unconcern, shocked me beyond words. The possibility of his having yellow fever did not occur to him just then; when it did, two days later, he declared he must have caught it at my autopsy room in the Military Hospital, or at "Las Animas" Hospital, where he had been two days before taking sick. Although we insisted that he should go to bed in his quarters, we could only get him to rest upon a lounge, until the afternoon, when he felt too sick and had to take to his bed.

Lazear and I were almost panic-stricken when we realized that Carroll had yellow fever. We searched for all possibilities that might throw the blame for his infection upon any other source than the mosquito which bit him four days before; Lazear, poor fellow, in his desire to exculpate himself, as he related to me the details of Carroll's mosquito experiment, repeatedly mentioned the fact that he himself had been bitten two weeks before without any effect therefrom and finally, what seemed to relieve his mind to some extent, was the thought that Carroll offered himself to feed the mosquito and that he held the tube upon his own arm until the work was consummated.

I have mentioned before that, as Lazear and I, vaguely hoping to find malarial parasites in Carroll's blood, sat looking into our microscopes that morning, the idea that the mosquito was what brought him down gradually took hold of our minds, but as our colleague had been exposed to infection in other ways, by visiting the yellow fever hospital "Las Animas," as well as the infected city of Havana, it was necessary to subject that same mosquito to another test and hence the inoculation of Private Dean, which is described in the opening chapter of this history.

Termination of the First Series of Mosquito Experiments. Death of Lazear.

The month of September, 1900, was fraught with worry and anxiety: what with Carroll's and Private Dean's attacks of yellow fever

and Major Reed's inability to return, Lazear and I were well-nigh on the verge of distraction. Private Dean was not married, but Carroll's wife and children, a thousand miles away, awaited in the greatest anguish the daily cablegram which told them the condition of the husband and father, who was fighting for life, sometimes the victim of the wildest delirium caused by consuming fever, at others almost about to collapse, until one day, the worst of the disease being over, the wires must have thrilled at our announcement, "Carroll out of danger."

Fortunately both he and Dean made an uninterrupted recovery, but we were still to undergo the severest trial, a sorrow compared to which the fearful days of Carroll's sickness lose all importance and dwindle almost into insignificance.

On the morning of the eighteenth my friend and classmate Lazear, whom in spite of our short intercourse I had learned to respect and in every way appreciate most highly, complained that he was feeling "out of sorts." He remained all day about the officers' quarters and that night suffered a moderate chill. I saw him the next day with all the signs of a severe attack of yellow fever.

Carroll was already walking about, though enfeebled by his late sickness, and we both plied Lazear with questions as to the origin of his trouble; I believe we affectionately chided him for not having taken better care of himself. Lazear assured us that he had not experimented upon himself, that is, that he had not been bitten by any of the purposely infected mosquitoes.

After the case of Dean so plainly demonstrated the certainty of mosquito infection, we had agreed not to tempt fate by trying any more upon ourselves, and even I determined that no mosquito should bite me if I could prevent it, since the subject of my immunity was one that could not be sustained on scientific grounds; at the same time, we felt that we had been called upon to accomplish such work as did not justify our taking risks which then seemed really unnecessary. This we impressed upon Major Reed when he joined us in October and for this reason he was never bitten by infected mosquitoes.

Lazear told us, however, that while at "Las Animas" Hospital the previous Thursday (five days before), as he was holding a test-tube with a mosquito upon a man's abdomen, some other insect which was flying about the room rested upon his hand; at first, he said, he was tempted to frighten it away, but, as it had settled before he had time to notice it, he decided to let it fill and then capture it; besides, he did not want to move in fear of disturbing the insect contained in his tube, which was feeding voraciously. Before Lazear could prevent it, the mosquito that bit him on the hand had flown away. He told us in his lucid moments, that, although Carroll's and Dean's cases had convinced him of the mosquito's rôle in transmitting yellow fever, the fact that no in-

fection had resulted from his own inoculation the month before had led him to believe himself, to a certain extent, immune.

How can I describe the agony of suspense which racked our souls during those six days? It seemed to us as though a life was being offered in sacrifice for the thousands which it was to contribute in saving. Across the span of thirteen years the memory of the last moments comes to me most vividly and thrilling, when the light of reason left his brain and shut out of his mind the torturing thought of the loving wife and daughter far away, and of the unborn child who was to find itself fatherless on coming to the world.

Tuesday, the twenty-fifth of September saw the end of a life full of promise; one more name, that of Jesse W. Lazear, was graven upon the portals of immortality. And we may feel justly proud for having had it, in any way, associated with our own.

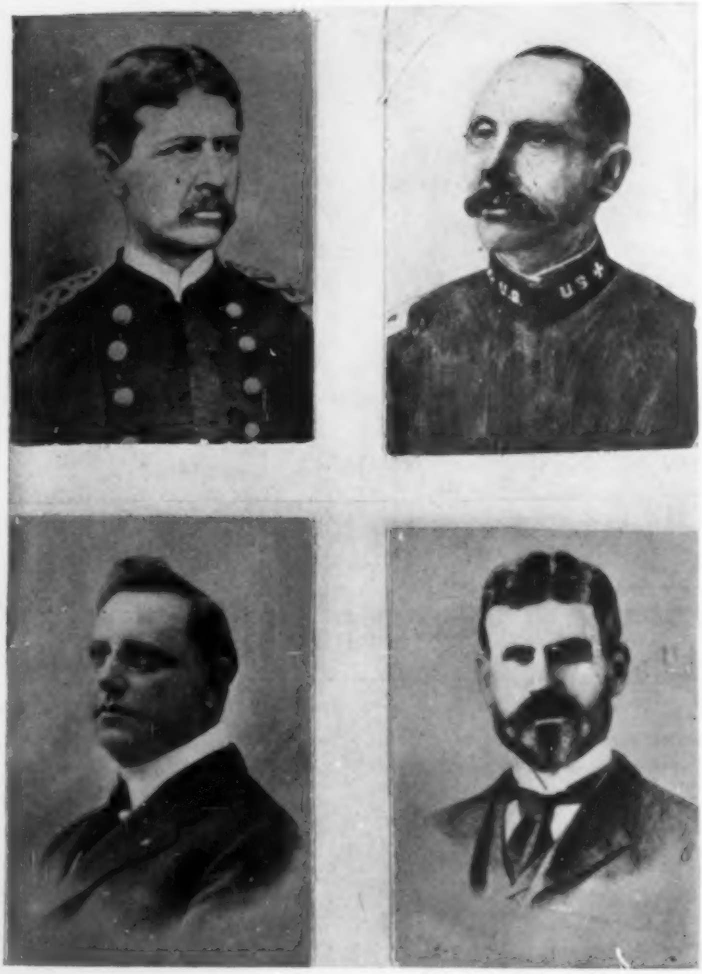
The state of mind in which this calamity left us may better be imagined than described. The arrival of Major Reed several days after in a great measure came to relieve the tensity of our nerves and render us a degree of moral support of which we were sorely in need.

Lazear's death naturally served to dampen our fruition at the success of the mosquito experiments, but, this notwithstanding, when the facts were known we were the subjects of much congratulation and the question whether the theory had been definitely demonstrated or not was the theme of conversation everywhere, about Havana and Camp Columbia particularly. We fully realized that three cases, two experimental and one accidental, were not sufficient proof, and that the medical world was sure to look with doubt upon any opinion based on such meager evidence; besides, in the case of Carroll, we had been unable to exclude the possibility of other means of infection, so that we really had but one case, Dean's, that we could present as clearly demonstrative and beyond question. In spite of this, we thought that the results warranted their presentation in the shape of a "Preliminary Note," and after all the data were carefully collected from Lazear's records and those at the Military Hospital, a short paper was prepared which the Major had the privilege to read at the meeting of the American Public Health Association, held on October 24, in the city of Indianapolis.

For this purpose Major Reed went to the States two weeks after his return to Cuba, and Carroll also took a short leave of absence so as to fully recuperate, in preparation for the second series of inoculations which we had arranged to undertake, after the Indianapolis meeting.

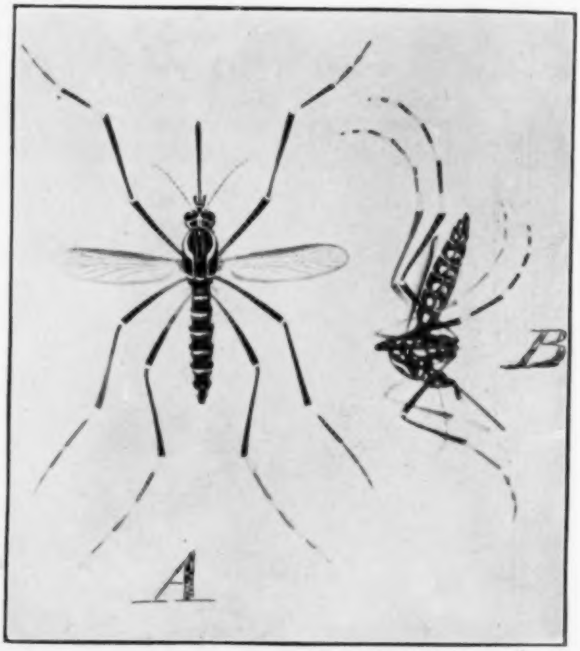
These inoculations, according to our program, were to be made upon volunteers who should consent to suffer a period of previous quarantine at some place to be selected in due time, away from any possibility of yellow fever.

It so happened then that I was left the only member of the board in Cuba and, under instructions from Major Reed, I began to breed mos-



THE UNITED STATES ARMY YELLOW-FEVER BOARD.

MAJOR WALTER REED. DR. JAMES CARROLL.
DR. ARISTIDES AGRAMONTE. DR. JESSE W. LAZEAR.



FEMALE YELLOW-FEVER MOSQUITO (*Stegomyia Calopus*). (*A*) The insect spread out to show the "lyre" marking on the back. (*B*) Position of the insect when ready to introduce its sting.

MALE YELLOW-FEVER MOSQUITO (*Stegomyia Calopus*). Showing the feathery antennæ peculiar to the sex in most mosquitoes.

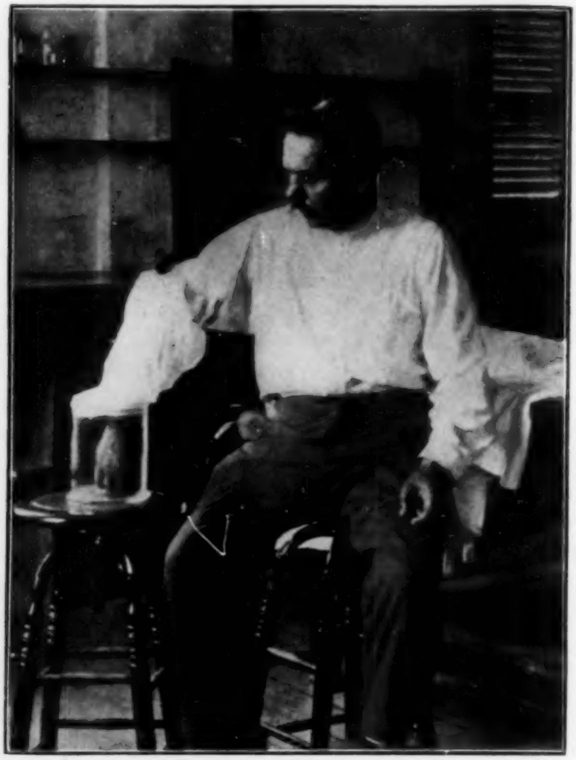
quitoes and infect them, as Lazear used to do, wherever cases occurred, keeping them at my laboratory in the Military Hospital No. 1. Major Reed had also asked me to look about for a proper location wherein to continue the work upon his return.

Origin and Development of the Mosquito Theory

The possible agency of insects in the propagation of yellow fever was thought of by more than one observer, from a very early period in the history of this disease. For instance, Rush, of Philadelphia, in 1797, noticed the excessive abundance of mosquitoes during that awful epidemic. Subsequently, several others spoke of the coincidence of gnats or mosquitoes and yellow fever, but without ascribing any direct relation to the one regarding the other. Of course, man-to-man infection through the sole intervention of an insect was a thing entirely inconceivable and therefore unthought of until very recently, and in truth the discovery, as far as yellow fever is concerned, was the result of a slow process of evolution of the fundamental fact, taken in connection with similar findings, in other diseases.

The earliest direct reference is found in the writings of Dr. Nott, of Mobile, Ala., who in 1848 suggested that the dissemination of the yellow-fever poison was evidently by means of some insect "that remained very close to the ground." But the first who positively pointed to the mosquito as the spreader of yellow fever, who showed that absence of mosquitoes precluded the existence of the disease and who prescribed the ready means to stamp it out, by fumigation and by preventing the bites of the insects, was Dr. Louis D. Beauperthuy, a French physician, then located in Venezuela. The writer has an original copy of his paper, published in 1853, where he fastens the guilt upon the domestic mosquitoes, believing, in accord with the prevailing teachings of medical science, that the mosquitoes infected themselves by contact or feeding upon the organic matter found in the stagnant waters where they are hatched, afterwards inoculating the victims by their sting. He recognized the fact that yellow fever is not contagious and therefore could not think of the possibility of man-to-man infection, as we know it to-day. The keenest observer was this man Beauperthuy, and, even at that benighted time in the history of tropical medicine, made most interesting studies of the blood and tissues, employing the microscope and the chemical reactions in his research. No one believed him, and a commission appointed to report upon his views said that they were inadmissible and all but declared him insane.

This field of investigation remained dormant for a comparatively long period of time. Meanwhile another medical writer, Dr. Greenville Dowell, mentions in 1876, that "if we compare the effect of heat and cold on gnats and mosquitoes with yellow fever, it will be difficult to believe it is of the same nature, as it is controlled by the same natural



METHOD OF FEEDING BLOOD TO MOSQUITOES; also used later for infecting them and for applying the insects to those who were inoculated.

laws." Soon after this, in 1879, the first conclusive proof of the direct transmission of a disease from man-to-man was presented by the father of tropical medicine, Sir Patrick Manson, with regard to filaria, a blood infection that often causes the repulsive condition known as elephantiasis and which the mosquito takes from man and after a short time gives over to another subject. This discovery attracted world-wide attention and many looked again towards the innumerable species of biting insects that dwell in the Tropic Zone, as possible carriers of the obscure diseases which also prevail in those regions.

In 1881, Dr. Carlos Finlay, of Havana, in an exhaustive paper read before the Royal Academy of Sciences, gave as his opinion that yellow fever was spread by the bites of mosquitoes "directly contaminated by stinging a yellow fever patient (or perhaps by contact with or feeding from his discharge)." This latter view he held as late as 1900, which, although correct in the main fact of the transmission of the germ from a patient to a susceptible person by the mosquito, the *modus operandi*, as he conceived it, was entirely erroneous.

Dr. Finlay, unfortunately was unable to produce experimentally

a single case of fever that could withstand the mildest criticism, so that at the time when the Army Board came to investigate the causes of yellow fever in Cuba, his theory, though practically the correct one, had been so much discredited, in a great measure by his own failures, that the best-known experts considered it as an ingenious, but wholly fanciful, one and many thought it a fit subject for humorous and sarcastic *repartee*. Finlay also believed, erroneously, that repeated bites of contaminated insects might protect against yellow fever and that the mosquitoes were capable of transmitting the germ to the next generation.

The wonderful discoveries of Theobald Smith, as to the agency of ticks in spreading Texas fever of cattle, and those of Ross and the Italian investigators who showed conclusively that malaria was transmitted by a species of mosquito, brought the knowledge of these various diseases to the point where the Army Board took up the investigation of yellow fever.

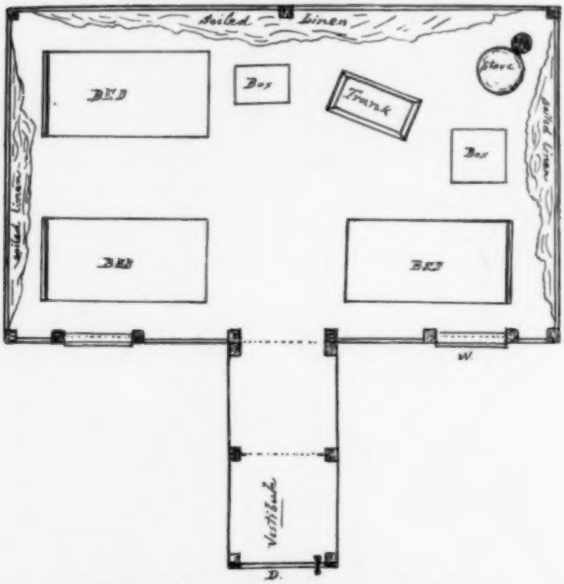
Second and Final Series of Mosquito Experiments

Major Reed came back to Havana in the early part of November, Carroll following a week after.

During their absence, I had been applying mosquitoes to yellow-fever patients at "Las Animas" Hospital, keeping them in my laboratory, as it was done at the beginning of the investigation; the season being more advanced, now and then a cold "norther" would blow and my insects suffered very much thereby, so that I had the greatest trouble in preventing their untimely death: to this may be added the difficulty met in feeding them blood, for now that I knew their sting was dangerous, unto death perhaps, I could not allow any indiscriminate biting, but had to select for the purpose individuals who had suffered an attack of the disease and were therefore immune.

The necessity for an experimental camp became more imperative as time passed, not only where proper quarantine and isolation could be established, but also where the insects intended for the inoculations might receive better care. This entailed considerable expense.

Fortunately for us, the military governor of the island at that time, Brigadier General Leonard Wood, was a man who had received a thorough medical training; broad and clear-minded, he fully appreciated the importance of what might be the outcome of our researches. We found in him the moral support which we so much needed and, further, he promptly placed at the disposal of the board sufficient funds with which to carry on the experiments to the end. I firmly believe that had other been the circumstances, had a more military and less scientific man been at the head of the government, the investigation would have terminated there and then, and many years would have passed, with



PLAN OF THE "INFECTED CLOTHING BUILDING" AT CAMP LAZEAR. Men who were susceptible to the disease slept many nights in this soiled linen room, without contracting yellow fever.

hundreds of lives uselessly sacrificed, before we could have attained our present remarkable sanitary triumphs.

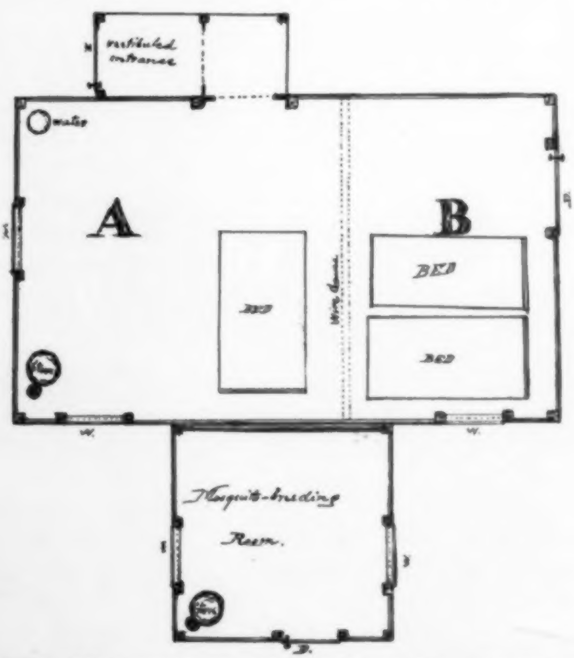
We immediately set about choosing a location for our camp. I had already looked over the ground, preferring the proximity of Camp Columbia, from where supplies could be easily obtained and because the Military Hospital there could be used for treating the cases that we intended to produce; I was therefore favorably impressed with the seclusion offered by a spot situated a short distance from the main road, in a farm, named San José, belonging to my friend Dr. Ignacio Rojas, of Havana. Major Reed decided upon this place after looking at many others in the neighborhood, so that on the twentieth of November we inaugurated our camp, which we named Camp Lazear, in honor to the memory of our dead colleague, consisting then of seven army tents, guarded by a military garrison, composed of men who had been carefully selected by virtue of their previous good record and their interest in the work to be undertaken.

Feeling that we had proved, to ourselves at least, the agency of the mosquito in yellow fever, it became our duty to disprove the theory, until then held as a certainty by many authorities, to the effect that the soiled bedding and clothing, the secretions and excreta of patients, were infectious and in some way carried the germ of the disease. We therefore designed a small wooden building, to be erected a short distance from the tents, with a capacity of 2,800 cubic feet. The walls and

ceiling were absolutely tight, the windows and vestibuled door screened and all precautions taken to prevent the entrance of insects.

Into this, called the "infected clothing building," three beds and a stove, to maintain a high tropical temperature, were introduced; also mattresses and pillows, underwear, pajamas, towels, sheets, blankets, etc., soiled with blood and discharges from yellow fever cases: these articles were put on the beds, hung about the room and packed in a trunk and two boxes placed there for the purpose.

The building was finished and equipped on November 30. That Friday evening, Dr. Robert P. Cook, U. S. Army, with two other American volunteers, entered it and prepared to pass the night: they had instructions to unpack the boxes and trunk, to handle and shake the clothing and in every way to attempt to disseminate the yellow fever poison, in case it was contained in the various pieces. We watched the proceedings from the outside, through one of the windows. The foul conditions which developed upon opening the trunk were of such a character that the three men were seen to suddenly rush out of the building into the fresh air; one of them was so upset that his stomach rebelled; yet, after a few minutes, with a courage and determination worthy only of such a cause, they went back into the building and passed a more or



PLAN OF THE "MOSQUITO BUILDING" AT CAMP LAZEAR. The man who for a short time occupied the bed in room marked "*A*" became infected by the bites of mosquitoes previously introduced, while those, equally susceptible, who occupied beds in section marked "*B*" remained in good health. Only a wire-screen partition separated the two compartments.

METHOD OF INFECTING MOSQUITOES, ORIGINALLY USED BY DR. LAZEAR. The tube containing the mosquito is applied upon the abdomen of the patient.

less sleepless night, in the midst of indescribable filth and overwhelming stench.

For twenty consecutive nights these men went through the same performance; during the day they remained together, occupying a tent near their sleeping quarters. Dr. Cook, by voluntarily undergoing such a test, without remuneration whatsoever, proved his faith in the mosquito theory; his demonstration of the harmless character of so-called infected clothing, in yellow fever, has been of the greatest importance. The other six men (two of them with Dr. Cook) who were subjected to this test, received each a donation of one hundred dollars for his services.

Many days even before the establishment of the experimental camp, the board had heard that several men who knew of our work were willing to submit to the inoculations and thus aid in clearing up the mystery of yellow fever. Two of these require special mention, John R. Kissinger, a private in the Hospital Corps of the Army, was the first to offer himself most altruistically, for, as he expressed it, his offer was made without any desire for pecuniary or other consideration and solely "in the interest of humanity and the cause of science," the other, J. J. Moran, a civilian employee, also stipulated as a condition that he was to receive no pay for his services. Both these men, in due time, suffered from yellow fever and until very recently had never obtained any reward for the great risk which they ran so voluntarily and praiseworthily.

Kissinger, who after several years' service in the army became disabled, is receiving a pension from the government; Moran, I hope, is still well and in the employ of the Isthmian Canal Commission, justly enjoying the friendship and confidence of his superior officers. The names of Kissinger and Moran should figure upon the roll of honor of the U. S. Army.

On the day the camp was definitely organized, Kissinger, who had not gone outside the military reservation for more than a month, moved into Camp Lazear and received his first bite from a mosquito which evidently was not "loaded" for, again on November 23, he was stung by the same insect without result. On December 5, five mosquitoes were applied, which brought about a moderate infection in three days. Moran was also bitten by mosquitoes which were supposed to be infected on November 26 and 29, both times unsuccessfully. As will be seen, he was infected later on.

By this time we had decided, the weather having cooled considerably, that it was better to keep the mosquitoes at a higher temperature and nearer to the men who were to be inoculated; therefore it was planned to put up another small wooden structure, which was to be known as the "Mosquito Building" in which an artificial temperature could be maintained; at my suggestion, the building was so designed that it might serve to infect individuals; by liberating infected mosquitoes on the inside and exposing some person to their stings, we could try to reproduce the infection as we felt it occurred in nature. Another reason for the mosquito house was the need to obviate the transportation of the insects from the Military Hospital, where I kept them, to our camp,

MOSQUITO CAGE (GLASS JAR WITH NETTING) USED FOR BREEDING INSECTS.

which could not be easily done without subjecting them to severe injury.

Upon one occasion I was taking four infected mosquitoes in the pocket inside my blouse from the laboratory in Havana to the experimental camp, accompanied by my attendant Private Loud; the horse which pulled my buggy, a rather spirited animal, becoming frightened at a steam roller, as we went around the corner of Colon Cemetery, started to race down the hill towards the Almendares River: Loud was thrown out by the first cavortings of the horse, who stood on its hind legs and jumped several times before dashing away, while I held tightly to the tubes in my pocket, as the buggy upset and left me stranded upon a sand pile in the middle of the road; the mosquitoes were quite safe, however, and upon my arrival at Camp Lazear I turned them over to Carroll for his subsequent care.

Another difficulty afterwards encountered was the scarcity of material susceptible to infection, for, although several men had expressed a willingness to be inoculated, when the time came, they all preferred the "infected clothing" experiment to the stings of our mosquitoes. We then thought best to secure lately landed Spaniards, to whom the probable outcome of the test might be explained and their consent obtained for a monetary consideration. Our method was as follows; as soon as a load of immigrants arrived, I would go to Tiscornia, the Immigration Station across the Bay of Havana, and hire eight or ten men, as day laborers, to work in our camp. Once brought in, they were bountifully fed, housed under tents, slept under mosquito-bars and their only work was to pick up loose stones from the grounds, during eight hours of the day, with plenty of rest between. In the meantime, as the days of observation passed, I carefully questioned them as to their antecedents, family history and the diseases which they might have suffered; those who had lived in Cuba or any other tropical country before were discarded at once and also those who were under age or had a family dependent upon them. When the selection was finally made, the matter of the experiment was put to them. Naturally, they all felt more or less that they were running the risk of getting yellow fever when they came to Cuba and so were not at all averse to allow themselves to be bitten by mosquitoes: they were paid one hundred dollars for this, and another equal sum if, as a result of the biting experiment, they developed yellow fever. Needless to say, no reference was made to any possible funeral expenses. A written consent was obtained from each one, so that our moral responsibility was to a certain extent lessened. Of course, only the healthiest specimens were experimented upon.

It so happened that some reporter discovered what we were about, or perhaps some invidious person misrepresented the facts; at any rate, on the twenty-first of November a Spanish newspaper appeared with flaring headlines denouncing the American doctors who were taking ad-

vantage of the poor immigrants and experimenting with them by injecting all sorts of poisons! It called upon the Spanish consul to look after his subjects. In view of this we felt that if such campaign continued, in a short time it would either make it impossible to secure subjects or cause diplomatic pressure to be exerted against the continuance of our experiments. It was thought best to "beard the lion in his den" so the three of us called upon the consul the following day. He was surprised to hear one of us address him in his own language, having taken us all for Americans on first sight, and when I explained to him our method of procedure and showed him the signed contracts with the men, being an intelligent man himself, he had no objections to offer and told us to go ahead and not bother about any howl the papers might make.

The first three cases (two of them Spaniards) which we produced came down with yellow fever within a very short period, from December 8 to 13; it will therefore not surprise the reader to know that when the fourth case developed on December 15, and was carried out of the camp to the hospital, it caused a veritable panic among the remaining Spaniards, who, renouncing the five hundred pesetas that each had in view, as Major Reed very aptly put it, "lost all interest in the progress of science and incontinentally severed their connection with Camp Lazear."

But there was a rich source to draw from, and the unexpected stampede only retarded our work for a short time. Our artificial epidemic of yellow fever was temporarily suspended while a new batch of susceptible material was brought in, observed and selected. The next case for that reason was not produced upon a Spaniard until December 30.

In the face of the negative experiments with supposedly contaminated articles, it rested with us to show how a house became infected and for this purpose the main part of the "mosquito building" was utilized.

This chamber was divided into two compartments by a double wire-screen partition, which effectually prevented mosquitoes on one side from passing to the other; of course there were no mosquitoes there to begin with, as the section of the building used for breeding and keeping them was entirely separated from the other, and there could be no communication between them.

On the morning of December 21, a jar containing fifteen hungry mosquitoes, that had previously stung cases of yellow fever, was introduced and uncovered in the larger compartment, where a bed, with all linen perfectly sterilized, was ready for occupancy. A few minutes after, Mr. Moran, dressed as though about to retire for the night, entered the room and threw himself upon the bed for half an hour; during this time two other men and Major Reed remained in the other compart-

ment, separated from Moran only by the wire-screen partition. Seven mosquitoes were soon at work upon the young man's arms and face; he then came out, but returned in the afternoon, when five other insects bit him in less than twenty minutes. The next day, at the same hour of the afternoon, Moran entered the "mosquito building" for the third time and remained on the bed for fifteen minutes, allowing three mosquitoes to bite his hands. The room was then securely locked, but the two Americans continued to sleep in the other compartment for nearly three weeks, without experiencing any ill effects.

Promptly on Christmas morning Moran, who had not been exposed to infection except for his entrance into the "mosquito building" as described, came down with a well-marked attack of yellow fever.

The temperature in this room, where these mosquitoes had been released, was kept rather high and a vessel with water was provided, where they might lay their eggs if so inclined, but notwithstanding all these precautions, it was subsequently found that the insects had been attacked by ants, so that by the end of the month only one of the fifteen mosquitoes remained alive.

It is hardly necessary to detail here how seven other men were subjected to the sting of our infected mosquitoes, of which number five developed the disease, but it may be interesting to note that two of these men had been previously exposed in the "infected clothing building" without their becoming infected, showing that they were susceptible to yellow fever after all.

The evidence so far seemed to show that the mosquito could only be infected by sucking blood of a yellow-fever patient during the first three days of the disease; to prove that the parasite was present in the circulating blood at that time we therefore injected some of this fluid taken from a different case each time, under the skin of five men: four of these suffered an attack of yellow fever as the result of the injection. The other one, a Spaniard, could not be infected either by the injection of blood or the application of mosquitoes which were known to be infected, showing that he had a natural immunity or, more likely, that he had had yellow fever at some previous time.

While selecting the Spaniards, it was often ascertained that they had been in Cuba before, as soldiers in the Spanish army usually, and the natural conclusion was that they had undergone infection; it was very seldom that any escaped during the Spanish control of the island.

Thus terminated our experiments with mosquitoes which, though necessarily performed on human beings, fortunately *did not cause a single death*; on the other hand, they served to revolutionize all standard methods of sanitation with regard to yellow fever. They showed the uselessness of disinfection of clothing and how easily an epidemic can be stamped out in a community by simply protecting the sick from the sting of the mosquitoes and by the extensive and wholesale destruction

of these insects which, added to the suppression of their breeding places, if thoroughly carried out, are the only measures necessary to forever rid a country of this scourge.

Besides keeping a sharp lookout against the importation of yellow fever cases, these are the simple rules that have kept the Panama Canal free and prevented the slaughter of hundreds of foreigners, so generally expected every year, in former times.

Since we made our demonstration in 1901, our work has been corroborated by various commissions appointed for the purpose, in Mexico, Brazil and Cuba, composed variously of Americans, French, English, Cuban, Brazilian and German investigators. Nothing has been added to our original findings; nothing has been contradicted of what we have reported, and to-day, after nearly thirteen years, the truths that we uncovered stand incontrovertible; besides, they have been the means of driving out yellow fever from Cuba, the United States (Laredo, Texas, 1903 and New Orleans, La., 1905), British Honduras and several cities of Brazil.

Of the Army Board only I remain. Lazear, as reported, died during the early part of our investigations; Reed left us in 1902 and Carroll only five years later. The reader may wonder of what benefit was it to us, this painstaking and remarkable accomplishment which has been such a blessing to humanity! See what the late Surgeon General of the U. S. Army had to say in his report (Senate Document No. 520, Sixty-first Congress, second session):

1. Major Walter Reed, surgeon, United States Army, died in Washington, D. C., from appendicitis, November 23, 1902, aged 51. His widow, Emilie Lawrence Reed, is receiving a pension of $125 a month.

2. Maj. James Carroll was promoted from first lieutenant to major by special act of Congress, March 9, 1907. He died in Washington, D. C., of myocarditis, September 16, 1907. His widow, Jennie H. Carroll, since his death, has received an annuity of $125 a month, appropriated from year to year in the Army appropriation bill.

3. Dr. Jesse W. Lazear, contract surgeon, United States Army, died at Camp Columbia, Cuba, of yellow fever, September 25, 1900. His widow, Mabel M. Lazear, since his death, has received an annuity of $125 a month appropriated from year to year in the Army appropriation bill.

4. Dr. Aristides Agramonte is the only living member of the board. He is professor of bacteriology and experimental pathology in the University of Habana and has never received, either directly or indirectly, any material reward for his share in the work of the board.

It is not for me to make any comments: the above paragraphs have all the force of a plain, truthful statement of facts. Perhaps it is thought that enough reward is to be found in the contemplation of so much good derived from one's own efforts and the feeling it may produce of innermost satisfaction and in forming the belief that one had not lived in vain. In a very great measure, I know, the thought is true.

THE EVOLUTION OF THE STARS AND THE FORMATION OF THE EARTH. IV

BY WILLIAM WALLACE CAMPBELL
DIRECTOR OF THE LICK OBSERVATORY, UNIVERSITY OF CALIFORNIA

THE PLANETESIMAL HYPOTHESIS

THE most elaborate structure yet proposed to explain the origin of the solar system is the planetesimal hypothesis by Chamberlin and Moulton. The energy which these investigators have devoted to formulating and testing this hypothesis, in the light of the principles of mechanics, has been commensurate with the importance of the subject. They postulate that the materials now composing the Sun, planets, and satellites, at one time existed as a spiral nebula, or as a great spiral swarm of discrete particles, each particle in elliptic motion about the central nucleus. The authors go further back and endeavor to account for the origin of the spiral nebula, but this phase of the subject is not vital to their hypothesis. However, it conduces to clearness in presenting their hypothesis to begin with the earlier process.

It may happen, once in a while, that two stars will collide. If the collision is a grazing one, they say, a spiral nebula will be formed. However, a fairly close approach of two stars will occur in vastly greater frequency and the effect of this approach will also be to form a spiral nebula or two such nebulæ. The authors recall that our Sun is constantly ejecting materials to a considerable height to form the prominences, and that the attractions of a great star passing fairly close to our solar system would assist this process of expulsion of matter from the Sun. A great outbreak or ejection of matter would occur not only on the side of our Sun turned toward the disturbing body, but on the opposite side as well, for the same reason that tides in our oceans are raised on the side opposite the Moon as well as on the side toward the Moon. As the Sun and disturbing star proceeded in their orbits, the stream of matter leaving our Sun on the side of the disturbing body would try to follow the other star; and the stream of matter leaving the other side of the Sun would shoot out in curves essentially symmetrical with those in the first stream. As the disturbing star approached and receded the paths taken by the ejected matter would be successively along curves such as are represented by the dotted lines in Fig. 28. At any given moment the ejected matter would lie on the two heavy lines. The matter would not be moving along the heavy lines, but nearly at right angles to them, in the directions that the lighter curves are pointing. As the ejections would not be continuous, but on the contrary

intermittent, because of violent pulsations of the Sun's body, there would be irregularities in the two spiral streamers. The materials drawn out of the Sun would revolve around it in elliptic orbits after the disturbing body had passed beyond the distance of effective disturbance,

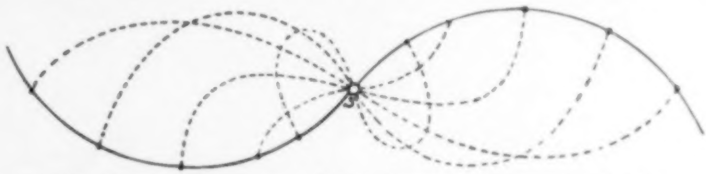


FIG. 28. THE ORIGIN OF A SPIRAL NEBULA, according to Moulton.

as illustrated in Fig. 29. The orbits of the different masses would have different sizes and different eccentricities. There would also be a wide distribution of finely-divided material between the main branches of the spiral. All of the widespread gaseous matter, hot when it left the

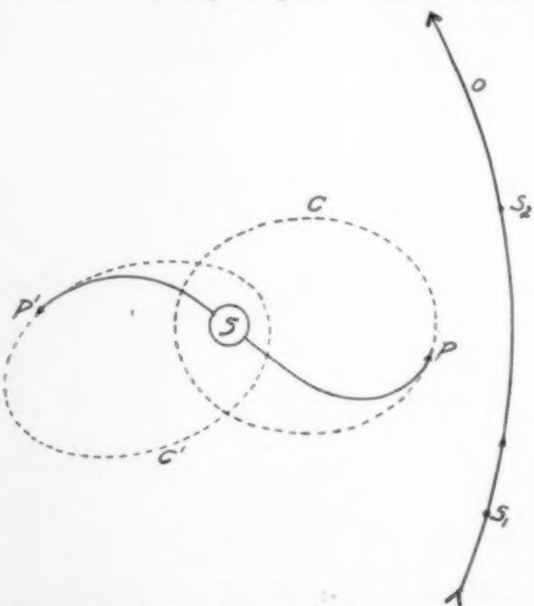


FIG. 29. THE ORIGIN OF ELLIPTICAL ORBITS OF MATTER EJECTED FROM THE SUN S AT THE TIME A STAR S_1—S_2 IS PASSING (Moulton).

Sun, would soon become cold, by expansion and radiation; and only the massive nuclei would remain gaseous and hot.

I see no reason to question the efficiency of this ingenious explanation of the origin of a spiral nebula: the close passage of two massive stars could, in my opinion, produce an effect resembling a spiral nebula, quite in accordance with Moulton's test calculations upon the subject. Some of the spirals have possibly been formed in this way (see Fig. 30);

FIG. 30. SPIRAL NEBULA III 55 PEGASI. Photographed at the Lick Observatory.

but that the tens of thousands of spirals known to exist in the sky have actually been produced in this manner is another question, and one which, in my opinion, is open to grave doubt. But to this point we shall return later.

There are marked advantages in starting the evolution of the solar system from a spiral nebula, aside from the fact that spirals are abundant, and therefore represent a standard product of development. The material is thinly and very irregularly distributed in a plane passing through the Sun, and the motions around the Sun are all in the same direction. The great difficulty in the Laplace hypothesis, as to the constancy of the moment of momentum, is here eliminated. There are well-defined condensations of nuclei at quite different distances from the Sun. According to this hypothesis the principal nuclei are the beginnings of the future planets. They draw into themselves the materials with which they come in contact by virtue of the crossings of the orbits of various sizes and various eccentricities. The growth of the planets is gradual, for the sweeping up and combining process must be excessively slow. The satellites are started from those smaller nuclei which happen to be moving with just the right speeds not to escape entirely the attractions of the principal nuclei, nor to fall into them. The planes of the planetary orbits and, in general, the planes of the satellite orbits should agree quite closely with each other, but they could differ and should differ from that of the Sun's equator.

The authors call attention to the fact that the Sun's equator is in-

clined at a small angle, 7°, to the common planes of the planetary system, and Chamberlin holds this to be one of the strong points in favor of the planetesimal hypothesis. He reasons thus: the star which passed close to our Sun and drew out the planetary materials in the form of spiral streams must have moved in the plane of the spiral; that is, in the plane of our planetary system. Some of the materials would be drawn out from our Sun only a very short distance and then fall back upon the Sun. Great tidal waves would be formed on opposite sides of the Sun, and these would try to follow the disturbing body. The effect of these waves and of the materials which fall back would be to change the Sun's original rotation plane in the direction of the disturbing body's orbital plane.

Now the chance for a disturbing star's passing around our Sun in a plane making a large angle, say from 45° to 90°, with the Sun's equator, is much greater than for a small angle 0° to 45°. The chances are greatest that the angle will be 90°. Only those disturbing stars which approach our Sun *precisely* in the plane of the Sun's equator could move around the Sun in this plane. All those approaching along any line parallel to the Sun's equatorial plane, but lying outside of this plane, and all those whose directions of approach make any angle whatever with the equatorial plane, would find it impossible to move in that plane. That the angle under this hypothesis is only 7° is surprising, though, as we are dealing with but a single case, we can not say, I think, that this militates either for or against the hypothesis. We are entitled to say only that unless the approach was so close as to cause disturbances in our Sun to relatively great depths, the angle referred to would have only one chance in ten or fifteen or twenty to be as small as 7°. Any disturbance which succeeded in taking out of the Sun only $\frac{1}{7}$ of 1 per cent. of its mass could scarcely succeed in shifting the axis of rotation of the remaining $99\frac{6}{7}$ per cent. very much, I think. If the angle were 30° or 50° or 80°, instead of 7°, the case for the planetesimal hypothesis would be somewhat stronger.

A remarkable fact concerning the Sun is that the equatorial region rotates once around in a shorter time than the regions in higher latitudes require. The rotation period of the Sun's equator is about 24 days; the period at latitude 45° is 28 days; and at 75°, 33 days. The planetesimal hypothesis attributes this equatorial acceleration to the falling back into the Sun of the materials which had been lifted out to a short distance by the disturbing body, and to the forward-rushing tide raised in the equatorial regions by the disturbing body. This may well have occurred. However, we must remember that the same phenomenon exists certainly in Jupiter and Saturn, and quite probably in Uranus and Neptune; that is, in all the bodies in the system that are gaseous and free to show the effect. It seems to be the result of a

principle which has operated throughout the solar system, not requiring, at least not directly requiring, the passage of a disturbing star. I think the most plausible explanation of this curious phenomenon is that great quantities of materials originally revolving around the Sun and around each of the planets have gradually been drawn into these bodies, by preference into their equatorial areas. Such masses of matter moving in orbits very close to these bodies must have traveled with speeds vastly higher than the surface speeds of the bodies. To illustrate, the rotational velocity of a particle now in the Sun's surface at the equator is approximately 2 km. per second. A small body revolving around the Sun close to his surface, rapidly enough to prevent its falling quickly upon the Sun, must have a velocity of more than 400 km. per second. If, now, this small body encounters some resistance it will fall into the Sun, and as it is traveling more than 200 times as rapidly as the solar materials into which it drops, it will both generate heat and accelerate the rotational velocity of the surrounding materials. In the same way the equatorial accelerations in Jupiter and Saturn can receive simple explanation. The point is not necessarily in opposition to the planetesimal hypothesis; but whatever the explanation, it ought to apply to the planet as well as to the Sun.

If the spiral nebulæ have been formed in accordance with Chamberlin and Moulton's hypothesis, the secondary nuclei in them must revolve in a great variety of elliptic orbits. The orbits would intersect, and in the course of long ages the separate masses would collide and combine and the number of separate masses would constantly grow smaller. Moulton has shown that *in general* the combining of two masses whose orbits intersect causes the combined mass to move in an orbit more nearly circular than the average orbit of the separate masses, and in general in orbit planes more nearly coincident with the general plane of the system. Accordingly, the major planets should move in orbits more nearly circular and more nearly in the plane of the system than do the asteroids; and so they do. If the asteroids should combine to form one planet the orbit of this planet should be much less eccentric than the average of all the present asteroid eccentricities, and the deviation of its orbit plane should be less than the average deviation of the present planes. We can not doubt that this would be the case. Mercury and Mars, the smallest planets, should have, according to this principle, the largest eccentricities and orbital inclinations of any of the major planets. This is true of the eccentricities, but Mars's orbit plane, contrarily, has a small inclination. Venus and the Earth, next in size, should have the next largest inclinations and eccentricities, but they do not; Venus's eccentricity is the smallest of all. The Earth's orbital inclination and eccentricity are both small. Jupiter and Saturn, Uranus and Neptune, should have the smallest orbital inclinations; their

average inclination is about the same as for Venus and the Earth. They should likewise have the smallest eccentricities. Neptune, the smallest of the four, has an orbit nearly circular; Jupiter, Saturn and Uranus have eccentricities more than 4 times those of Venus and the Earth. Considering the four large planets as one group and the four small planets as another group, we find that the inclinations of the orbits of the two groups, per unit mass, are about equal; but the average eccentricity of the orbits of the large planets, per unit mass, is 2¼ times that of the orbits of the small planets.[10] The evidence, except as to the asteroids and Mercury, is not favorable to the planetesimal hypothesis, unless we make special assumptions as to the distribution of materials in the spiral nebulæ.

The fact that the disturbing body drew 225 times as much matter a great distance to form the four large planets as it drew out a short distance to form the four small planets and the asteroids seems difficult of explanation on the planetesimal hypothesis. However, this distribution of matter is at present a difficulty in any of the hypotheses. The planetesimal hypothesis explains well all west to east rotations of the planets on their axes, but to make Uranus rotate nearly at right angles to the plane of the system, and Neptune in a plane inclined 135° to the plane of the system, is a difficulty in any of the hypotheses, unless special assumptions are made to fit each case.

The authors succeed well, I think, in showing that the satellites should prefer to revolve around their planets in the direction of the planetary revolution and rotation, especially for close satellites, and, on the basis of special assumptions, in the reverse direction for satellites at a greater distance. They show that the chances favor small eccentricities for satellites revolving about their planets in the west to east, or direct sense, and large eccentricities for satellites moving in retrograde directions. The inner satellite of Mars and the rings of Saturn make no special difficulty under the planetesimal hypothesis.

The evidence of the comets, as bona fide members of the solar system which approach the Sun almost, and perhaps quite, indifferently from all directions, is that the volume of space occupied by the parent structure of the system was of enormous dimensions, both at right angles to the present principal plane of the system and in that plane. We are accustomed to think of the spiral nebulæ as thin relatively to their major diameters. To this extent the planetesimal hypothesis does not furnish a good explanation of the origin of comets, unless we assume that a small amount of matter was widely scattered in all directions around the parent spiral; and this conception leads to some apparent difficulties. The origin of the comets is difficult to explain under any of the hypotheses.

[10] The average eccentricity of the orbits of the four inner planets (per unit mass) is 0.0221, and of the four outer planets is 0.0489.

Résumé of Hypotheses

Kant's hypothesis had the great defect of trying to prove too much. It started from matter *at rest*, and came to grief in trying to give a motion of rotation to the entire mass through the operation of internal forces alone—an impossibility. Kant's idea of nuclei or centers of gravitational attraction, scattered here and there throughout the chaotic mass, which grew into the planets and their satellites, is very valuable.

Laplace's hypothesis had the great advantage of starting with an extended mass already in rotation, but it violated fatally the law of constancy of moment of momentum. We should expect this hypothesis to create a solar system free from irregularities, very much as if it were the product of an instrument-maker's precision lathe. The solar system as it exists is a combination of regularities and many surprising irregularities.

Chamberlin and Moulton's hypothesis has the advantage of a parent mass in rotation, practically in a common plane, and with the materials distributed at distances from the nucleus as nearly in harmony with the known distribution of matter in the solar system as we care to have them, except perhaps as to the comets. In effect it retains all the advantageous qualities of Kant's proposals. It seems to have the flexibility required in meeting the irregularities that we see in our system.

Concerning the Origin of Spiral Nebulæ

I think it is very doubtful whether the spiral nebulæ have in general been formed by the close approaches of pairs of stars, as the authors have postulated for the assumed solar spiral.[11] The distribution of the spirals seems to me to negative the idea. To witness the close approach of two stars we must look in the direction where the stars are. To the best of present-day knowledge the stars are in a spheroid whose longer axes are coincident with the plane of the Milky Way. If this is so, the close approach of pairs of stars should occur preeminently in the Milky Way, and we should find the spirals prevailingly in and near the Milky Way. This is precisely where we do not find them. In fact, they seem to abhor the Milky Way. The new stars, which are credibly explained as the products of collisions of stars with nebulæ, are found

[11] It would seem that all rotating nebulæ should in reality possess some of the attributes of spiral motion. Whether the spiral structure should be visible or invisible to a terrestrial observer would depend upon the sizes and distances of the nebulæ, upon the distribution of materials composing them, and perhaps upon other factors. See developed the hypothesis that spiral nebulæ owe their origin to the collision of two nebulæ. Collisions of this kind could readily occur because of the enormous dimensions of the nebulæ, and motions of rotation and consequently spiral structure might readily result therefrom. The abnormally high speeds of the spiral nebulæ are apparently a very strong objection to the hypothesis.

preeminently in the Milky Way and almost negligibly in the regions outside of the Milky Way. Again, the spirals are believed to be, on the whole, of enormous size. They are too far away to let us measure their distances by the usual methods, and they move too slowly on the surface of the sphere to have let us determine their proper motions. Slipher's recent work with a spectrograph seems to show that the dozen spirals observed by him are moving with high speeds of approach and recession; from 300 km. per second approach in the case of the Andromeda nebula to 1,100 km. per second recession in the case of several objects. If the spirals are moving at random their speeds at right angles to the line of sight must be even greater than their speeds of approach and recession. Unless they are very distant bodies their proper motions should be detected by observations extending over only a few years. My colleague Curtis has this year compared recent photographs of some 25 spirals with photographs of the same object made by Keeler fifteen years ago. They reveal no appreciable proper motions, or rotations. In this same interval Neptune has revolved more than 30°. Slipher has recently measured the rotational speed of one "spindle" nebula, believed to be a spiral. He finds it to be enormously rapid; no motions in the solar system approach it in magnitude. The evidence is to the effect that the spirals are in general very far away;[12] perhaps on or beyond the confines of our stellar system, but not certainly so. Accordingly, we are led to believe that the spirals studied thus far have diameters 20 times or 100 times, or in some cases several thousand times, the diameter of our solar system. It is difficult to avoid the conclusion that in general they are immensely more massive than is our solar system. The spiral which has been assumed as the forerunner of our system must have been of diminutive size as compared with the larger and brighter spirals which we see to-day.

We are sadly in need of information concerning the constitution of the spiral nebulæ. Their spectra appear to be prevailingly of the solar type, except that a very small proportion contain some bright lines in addition to the continuous spectrum. So far as their spectra are concerned, they may be great clusters of stars, or they may consist each of a central star sending its light out upon surrounding dark materials and thus rendering these materials visible to us. The first alternative is unsatisfactory, for all parts of spirals have hazy borders, as if the structure is nebulous or consists of irregular groups of small masses; and the second alternative is unsatisfactory, for in many spirals the most outlying masses seem to be as bright as masses of the same areas situated only one half as far from the center, whereas in general the inner area should be at least four times as bright as the outer area.

[12] Bohlin found a parallax of 0″17 for the Andromeda Nebula, and Lampland thinks that Nebula N.G.C. 4594 has a proper motion of approximately 0″05 per annum.

All astronomers are ready to confess that we do not know much about the conditions existing in spiral nebulæ.

The Earth-Moon System

Our Earth and Moon form a unique combination in that they are more nearly of the same size than are any other planet and its satellites in our system. It required a 26-inch telescope on the Earth to discover the tiny moons of Mars; but an astronomer on Mars does not need any telescope to see the Earth and Moon as a double planet—the only double planet in the solar system.

According to the Kantian school of hypotheses the Earth and Moon owe their unique character to the accident that two centers of condensation—two nuclei—not very unequal in mass, were formed close to each other and were endowed with or acquired motions such that they revolved around each other. They drew in the surrounding materials; one of the two bodies got somewhat the advantage of the other in gravitational attraction; it succeeded in building itself up more than the other nucleus did; and the Earth and the Moon were the result.

According to the Laplacean hypothesis, on the contrary, the Earth and Moon were originally one body, gaseous and in rotation. This ball of gas radiated heat, diminished in size, rotated more and more rapidly, and finally abandoned a ring of nebulosity which later broke up and eventually condensed into one mass called the Moon. The central mass composed the Earth. It is a curious fact that Venus, which is only a shade smaller than the Earth, should not have divided into two bodies comparable with the Earth and Moon. Have the tides on Venus produced by the Sun always been strong enough to keep the rotation and revolution periods equal, as they are thought to be now, and thus to have given no opportunity for a rapidly rotating Venus to divide into two masses?

A third hypothesis of the Moon's origin is due principally to Darwin. He and Poincaré have shown that a great rotating mass of fluid matter, such as the Earth-Moon could be assumed to have been, by cooling, contracting and increasing rotation speed, would, under certain conditions thought to be reasonable, become unstable and eventually divide into two bodies revolving around their common center of mass, at first with their surfaces nearly in contact. Here would begin to act a tide-raising force which must have played, according to Darwin's deductions, a most important part in the further history of the Earth and Moon. The Earth would produce enormous tides in the Moon, and the Moon much smaller tides in the Earth. Both bodies would contract in size, through loss of heat, and would try to rotate more and more rapidly. The two rotating bodies would try to carry the matter in the tidal waves around with the rest of the materials

in the bodies, but the pull of each body upon the wave materials in the other would tend to slow down the speed of rotation. The tidal resistance to rotation would be slight if the bodies at any time were attenuated gaseous masses, for the friction within the surface strata would be slight. Nevertheless, there would eventually be a gradual slowing down of the Moon's rotation, a gradual slowing down of the Earth's rotation, and a slow increase in the distance between the two bodies. In other words, the Moon's day, the Earth's day and our month would gradually increase in length. Carried to its logical conclusion, the Moon would eventually turn the same face to the Earth, the Earth would eventually turn the same face to the Moon, and the Earth's day and the Moon's day would equal the month in length. The central idea in this logic is as old as Kant: in 1754 he published an important paper in which he said that tidal interactions between Earth and Moon had caused the Moon to keep the same face turned toward us, that the Earth's day was being very slowly lengthened, and that our planet would eventually turn the same face to the Moon. Laplace, a half-century later, proposed the action of such a force in connection with the explanation of lunar phenomena, and Helmholtz, just 100 years after Kant's paper was published, lent his support to this principle; but Sir George Darwin has been the great contributor to the subject. His popular volume, "The Tides," devotes several chapters to the effects of tidal friction upon the motions of two bodies in mutual revolution. We must pass over the difficult and complicated intermediate steps to Darwin's conclusions concerning the Earth and Moon, which are substantially as follows: the Earth and Moon were originally much closer together than they now are: after a very long period of time, amounting to hundreds of millions of years, the Moon will revolve around the Earth in 55 days instead of in 27 days as at present; and the Moon and Earth will then present the same faces constantly to each other. The estimated period of time required, and the final length of day and month, 55 days, are of course not insisted upon as accurate by Darwin.

These tidal forces were unavoidably active, it matters not if the Earth and Moon were originally one body, as Laplace and Darwin have postulated, or originally two bodies, growing up from two nuclei, in accordance with the Kantian school. Whether these forces have been sufficiently strong to have brought the Earth and Moon to their present relation, or will eventually equalize the Moon's day, the Earth's day, and the month, is a vastly more difficult question. Moulton's researches have cast serious doubt upon this conclusion. All such investigations are enormously difficult, and many questionable assumptions must be made if we seek to go back to the Moon's origin, or forward to its ultimate destiny.

Tidal waves, in order to be effective in reducing the rotational speed of a planet, must be accompanied by internal friction; and this requires that the planet be to some extent inelastic. It was the view of Darwin and others that the viscous state of the Earth and Moon permitted wave friction to come into play. Michelson has recently proved that the Earth has a high degree of elasticity. It deforms in response to tidal forces, but quickly recovers from the action of these forces. It therefore seems that the rate of tidal evolution of the Earth-Moon system at present and in the future must be extremely slow, and possibly almost negligible. What the conditions within the Earth and Moon were in the distant past is uncertain, but these bodies probably passed through viscous stages which endured through enormously long periods of time. No one seriously doubts that Jupiter, Saturn, Uranus and Neptune are now largely gaseous, and that they will evolve, through various degrees of viscosity, into the solid and comparatively elastic state. It is natural to assume that the Earth has already passed through an analogous experience.

The Moon turns always the same hemisphere toward the Earth. Observations of Venus and Mercury are prevailingly to the effect that those planets always turn the same hemispheres toward the Sun. Many, and perhaps all, of the satellites of Jupiter and Saturn seem to turn the same hemispheres always toward their respective planets. This widely prevailing phenomenon is no doubt due to a widely prevailing cause, which astronomers have all but unanimously attributed to tidal action.

Binary Star Systems

That an original mass actually divided to form the Earth and Moon, according to the Laplacian or the Darwin-Poincaré principle, seems to be extremely doubtful, especially on account of their diminutive sizes, and I greatly prefer to think that the Earth and Moon were built up from two nuclei; but that very much greater masses, masses larger on the average than our Sun, composing highly attenuated stars, have divided each into two masses to form many or most of our double stars, I firmly believe. The two component stars would in such a case at first revolve around each other with their surfaces almost or quite in contact. Tidal forces would very gradually cause the bodies to move in orbits of larger and larger size, with correspondingly longer periods of revolutions, and the orbits would become constantly more eccentric. While these processes were under way the component bodies would be radiating heat and growing smaller, and their spectra would be changing into the more advanced types. We can not hope to watch such changes as they occur, but we can, I think, find abundant illustrations of these processes in the double stars. I have given reasons for believing that one star in every two and one half, as a minimum

proportion, is not the single star which it appears to be to the eye or in the telescope, but is a system of two or more suns in mutual revolution. The formation of double stars, therefore, is not a sporadic process: it is one of the straightforward results of the evolutionary process.

Some of the variable stars offer strong evidence as to the early life of the double stars. The so-called β Lyræ variables vary continuously in brightness, as if they consist in each case of two stars so close together that their surfaces are actually in contact in some pairs and nearly in contact in others, so that from our point of view the two stars mutually eclipse each other. When the two stars are in line with us we have minimum brightness. When they have moved a quarter-revolution farther, and the line joining them is at right angles to our line of sight, so to speak, we have maximum brightness. *In every known case the β Lyræ pairs of stars have spectra of the very early types.* Some of them even contain bright lines in their spectra. The densities of these great stars are known to be exceedingly low, in some cases much lower on the average than that of the atmosphere which we breathe.

About 80 Algol variable stars are known. These are double stars whose light is constant except during the short time when one of the components in each system passes between us and the other component. All double stars would be Algol variables if we were exactly in the planes of their orbits. That so few Algols have been observed amongst the tens of thousands of double stars, is easily explained. The two component stars in the few known Algol systems are so great in diameter, in proportion to the size of their orbits, that eclipses are observable throughout a wide volume of space, and the eclipses are of long duration relatively to the revolution period. Their densities are, so far as we have been able to determine them, on an average less than 1/10th of the Sun's density. Let us note well that their spectra, so far as we have been able to determine them, are of the early types; mostly helium and hydrogen stars, and a very few of the Class F, intermediate between the hydrogen and solar stars. There are no known Algols of the Classes G, K, and M: these stars are very condensed and therefore small in size, as compared with stars of Classes B and A; and the components of double stars of these classes are on the average much denser and therefore smaller in size than the components in Classes B and A double stars; the components are much farther apart in Classes G to M doubles than in Classes B and A doubles; and for these reasons eclipses in Classes G to M doubles occur but rarely for observers scattered throughout space. It is difficult to avoid the conclusion that the components of double stars separate more and more widely with the progress of time. The conclusions which we have earlier drawn from visual double stars are in full harmony with the argument.

It is agreed by all, I think, that tidal action has been responsible for at least a part of the separation of the Earth and Moon, for at least a part of the gradual separation of the components of double stars, and for at least a part of the eccentricity of their orbits. See's investigations of 25 years ago led him to the conclusion that this force is sufficient to account for all the observed separation of the components of double stars, and for the well-known high eccentricities of their orbits. In recent years Moulton and Russell have seriously questioned the sufficiency of this force to account for the major part of the separation and eccentricity in the double star systems. I think, however, that if the tidal force is not competent to account for the observed facts as described, some other separating force or forces must be found to supply the deficiency.

The Formation of the Earth

Does the condition of the Earth's interior give evidence on the question of its origin? There are certain important facts which bear upon the problem.

1. The evidence supplied by the volcanoes, by the hot springs, and by the rise in temperature as we go down in all deep mines, is unmistakably to the effect that there is an immense quantity of heat in the Earth's interior. Near the surface the temperature increases at the average of 1° Centigrade for every 30 meters of depth. If this rate were maintained we should at 60 km. in depth arrive at a temperature high enough to melt platinum, the most refractory of the known metals. What the law of temperature-increase at great depths is we do not know, but the temperature of the Earth's deep interior must be very high.

2. The pressures in the Earth increase from zero at the surface to the order of 3,000,000 atmospheric pressures at the center. We know that rock structure, or iron or other metals, can be slightly compressed by pressure, but the experiments at very high pressures, notably those conducted by Bridgman, give no indications that matter under such pressures breaks down and obeys different or unknown laws. It should be said, however, that laboratory pressure-effects alone are not a safe guide as to conditions within the Earth, where high pressures are accompanied by high temperature. Unfortunately it has not been found possible to combine the high-temperature factor with the high-pressure factor in the laboratory experiments. It is well known that the melting points of metals, including rocks, increase with increase of pressure; and although the temperatures in the Earth's interior are very high, it is easy to conceive that the materials of the Earth's interior are nevertheless in the solid state, or that they act like solids, because of the high pressures to which they are subjected.

3. The specific gravity of the entire Earth is 5.5 on the scale of water as one, whereas the density of the stratified rocks averages only

2.75; that is, the stratified rocks have but one half the density of the Earth as a whole. The basaltic rocks underlying the stratified attain occasionally the density 3.1, and perhaps a little higher. It follows absolutely that the density of the materials of the Earth's interior must be considerably in excess of 5.5. If the interior is composed chiefly of substances which are plentiful in the Earth's surface strata, our choice of materials which principally compose the interior is reduced to a few elements, notably the denser ones.

4. The observed phenomena of terrestrial precession can not be explained on the basis of an Earth with a thin solid surface shell and a liquid interior, for the attractions of the Moon and Sun upon the Earth's equatorial protuberance would cause the surface shell to shift over the fluid interior, instead of swinging the entire Earth.

5. If the Earth consisted of a thin solid shell upon a liquid interior there would be tides in the liquid interior, the crust would yield to these tides almost as if it were composed of rubber, and the ocean tides would be only an insignificant amount larger than the land tides. As a result we should not see the ocean tides; their visibility depends upon the contrast between the ocean tides and the land tides. If the Earth were absolutely unyielding from surface to center the ocean tides would be relatively 50 per cent. higher than we now see them. The conclusion from these facts is that the Earth yields to the tidal forces a little less than if it were a solid ball of steel, supposing that the well-known rigidity and density existed from surface to center of the ball. This result is established by Darwin's and Schweydar's studies of ocean tides, by studies of the tides in the Earth's surface strata made by Hecker, Paschwitz and others, and by Michelson's recent extremely accurate comparison of land and water tides. Michelson's results establish further that the Earth is highly elastic: though distortion is resisted, there is yielding, but the original form is recovered quickly, almost as quickly as a perfectly elastic body would recover.

6. Some 25 years ago it was discovered by Küstner that the latitudes of points on the Earth's surface are changing slowly. Chandler proved that these variations pass through their principal cycle in a period of 427 days. The entire Earth oscillates slightly in this period. The earlier researches of Euler had shown that the Earth would have a natural oscillation period of 305 days provided it were an absolutely rigid body. Newcomb showed that the period of oscillation would be 441 days if the Earth had the rigidity of steel. As the observed oscillation requires 427 days, Newcomb concluded that the Earth is slightly more rigid than steel.

7. The first waves from a very distant earthquake come to us directly through the Earth. The observed speeds of transmission are the greater, in general, the more nearly the earthquake origin is exactly on

the opposite side of the Earth from the observer; that is, the speeds of transmission are greater the nearer the center of the Earth the waves pass. Now, we know that the speeds are functions of the rigidity and density of the materials traversed. The observed speeds require for their explanation, so far as we can now see, that the rigidity of the Earth's central volume be much greater than that of steel, and the rigidity of the Earth's outer strata considerably less than that of steel. Wiechert has shown that a core of radius 4,900 km. whose rigidity is somewhat greater than that of steel and whose average density is 8.3, overlaid by an outer stony shell of thickness 1,500 km. and average density 3.2, would satisfy the observed facts as to the average density of the Earth, as to the speeds of earthquake waves, as to the flattening of the Earth,—assuming the concentric strata to be homogeneous in themselves,—and as to the relative strengths of gravity at the Poles and at the Equator. The dividing line, 1,500 km. below the surface—1,600 km. would be just one fourth of the way from the surface to the center—places a little over half the volmue in the outer shell and a little less than half in the core. Wiechert did not mean that there must be a sudden change of density at the depth of 1,500 km., with uniform density 8.3 below that surface and uniform density 3.2 above that surface. The change of density is probably fairly continuous. It was necessary in such a preliminary investigation to simplify the assumptions. The observational data are not yet sufficiently accurate to let us say what the law of increase in density and rigidity is as we pass from the surface to the center.

8. The phenomena of terrestrial magnetism indicate that the distribution of magnetic materials in the Earth is far from uniform or symmetrical; the magnetic poles are distant from the Earth's poles of rotation; the magnetic poles are not opposite each other; the lines of equal intensity as to all the magnetic components involved run very irregularly over the Earth's surface. There is reason to believe that iron in the deep interior of the Earth, in view of its high temperature, is devoid of magnetic properties, but we must not state this as a fact. We know that iron is very widely, but very irregularly spread throughout the Earth's outer strata. Whatever may be the main factors in making the Earth a great magnet, to whatever extent the rotation factor may be important, the Earth's magnetic properties point strongly to a very irregular distribution of magnetic materials in the outer strata where the temperatures are below that at which magnetic materials commonly lose their polarity.

9. Irregularities in the direction of the plumb-line and in the force of gravity as observed widely and accurately over the Earth's surface indicate that the surface strata are very irregular as to density. To harmonize the observed facts Hayford has shown the need of assuming

that the heterogeneous conditions extend down to a depth of 122 km. from the surface. Below that level the Earth's concentric strata seem to be of approximately uniform densities.

10. The radio active elements have been found by Strutt and others in practically all kinds of rock accessible to the geologists, but they are not found in significant quantities in the so-called metals which exist in a pure state. These radioactive elements are liberating heat. Strutt has shown that if they existed down to the Earth's center in the same proportion that he finds in the surface strata they would liberate a great deal more heat than the body of the Earth is now radiating to outer space. The conclusion is that they are restricted to the strata relatively near the Earth's surface, and are not in combination with the materials composing the Earth's core. They have apparently found some way of coming to the higher levels. Chamberlin suggests that as they liberate heat they would raise surrounding materials to temperatures above the normals for their strata, and that these expanded materials would embrace every opportunity to approach the surface of the Earth, carrying the radioactive substances with them.

The evidence is exceedingly strong, and perhaps irresistible, to the effect that the Earth is now solid, or acts like a solid, from surface to center, with possibly local, but on the whole negligible, pockets of molten matter here and there; and further, that the Earth existed in a molten, or at the least a thickly plastic, state throughout a long part of its life. The nucleus, whether gaseous or meteoric, from which I believe it has grown, may have been fairly hot or quite cold, and the materials which were successively drawn into the nucleus may have been hot or cold: heat would be generated by the impacts of the incoming materials; and as the attraction toward the center of the mass became strong, additional heat would be generated in the contraction process. The denser materials have been able, on the whole, to gravitate to the center of the structure, and the lighter elements have been able, on the whole, to rise to and float upon the surface very much as the lighter impurities in an iron furnace find their way to the surface and form the slag upon the molten metal. The lighter materials which in general form the surface strata are solid under the conditions of solids known to us in every-day life. The interior is solid or at least acts as a solid, because the materials, though at high temperatures, are under stupendous pressures. If the pressures were removed the deep-lying materials would quickly liquefy, and probably even vaporize.

If the Earth grew from a small nucleus to its present size by the extremely gradual drawing-in of innumerable small masses in its neighborhood, the process would always be slow; much slower at first when the small nucleus had low gravitating powers, more rapid when the body was of good size and the store of materials to draw upon plentiful,

and gradually slower and slower as the supply of building materials was depleted. Meteoric matter still falls upon and builds up the Earth, but at so slow a rate as to increase the Earth's diameter an inch only after the passage of hundreds of millions of years. If the Earth grew in this manner, the growth may now be said to be essentially complete, through the substantial exhaustion of the supply of materials.

Whether the Earth of its present size was ever completely liquefied, that is, from center to surface, at one and the same time, is doubtful. The lack of homogeneity, as indicated by the plumb-line, gravity, terrestrial magnetism and radiaoctive matter, extending in a perceptible degree down to 122 km., and quite probably in lesser and imperceptible degree to a much greater depth, is opposed to the idea.

Solidification would respond to the fall of temperature down to the point required under the existing high pressures, and it is probable that the solidification began at the center and proceeded outwards. It is natural that the plastic state should have developed and existed especially during the age of most rapid growth, for this would be the age of most rapid generation of heat. Later, while the rate of growth was declining, the body could probably have solidified slowly and successively from center out to surface. In later slow depositions of materials, the denser substance would not be able to sink down to the deepest strata: they must lie within a limited depth and horizontal distance from where they fell, and the outer stratum of the Earth would be heterogeneous in density.

The simplest hypothesis we can make concerning the Earth's deep interior is that the chief ingredient is iron; perhaps a full half of the volume is iron. The normal density of iron is 7.8, and of rock formations about 2.8. If these are mixed, half and half, the average density is 5.3. Pressures in the Earth should increase the density and the heat in the Earth should decrease the density. The known density of the Earth is 5.5. We know that iron is plentiful in the Earth's crust, and that iron is still falling upon the Earth in the form of meteorites. The composition of the Earth as a whole, on this assumption, is very similar to the composition of the meteorites in general. They include many of the metals, but especially iron, and they include a large proportion of stony matter. Iron is plentiful in the Sun and throughout the stellar universe. Why should it not be equally plentiful in the materials which have coalesced to form the Earth? It is difficult to explain the Earth's constitution on any other hypothesis.

The Earth's form is that which its rotation period demands. Undoubtedly if the period has changed, the form has changed. Given a little time, solids under great pressure flow quite readily into new forms. Now any great slowing-down of the Earth's rotation period within geological times would be expected to show in the surface features. The

strata should have wrinkled, so to speak, in the equatorial regions and stretched in the polar regions, if the Earth changed from a spheroid that was considerably flatter than it now is, to its present form. Mountains, as evidence of the folding of the rock strata, should exist in profusion in the torrid zone, and be scarce in or absent from the higher latitudes of the Earth. Such differential effects do not exist, and it seems to follow that changes in the Earth's rotation period and in its form could have been only slight while the stratification of our rocks was in progress.

Geologists estimate from the deposition of salt in the oceans, and from the rates of denudation and sedimentation, that the formation of the rock strata has consumed from 60,000,000 to 100,000,000 years. If the Earth had substantially its present form 80,000,000 years ago we are safe in saying that the period of time represented in the building up of the Earth from a small nucleus to its present dimensions has been vastly longer, probably reckoned in the thousands of millions of years.

For more than a century past the problem of the evolution of the stars, including the solar system and the Earth, has occupied the central place in astronomical thought. No one is bold enough to say that the problem has been solved. The chief difficulty proceeds from the fact that we have only one Earth, one solar system and one stellar system available for tests of the hypotheses proposed; we should like to test them on many systems, but this privilege is denied us. However, the search for the truth will undoubtedly proceed at an ever increasing pace, partly because of man's desire to know the truth, but chiefly, as Lessing suggested, because the investigator finds an irresistible satisfaction in the process. There is always with him the certainty that the truth is going to be incomparably stranger and more interesting than fiction.

A METRICAL TRAGEDY

By Dr. JOS. V. COLLINS

STEVENS POINT, WIS.

THE war in Europe has opened up a large field of trade in South America. Three things especially stand in the way of its development, viz., the absence of a proper credit system, the failure to make goods of the kind demanded and third, the use of our antiquated system of weights and measures, all the South American countries employing the metric system. Of these three obstructing influences, the first two are in a fair way to be obviated soon; not so the last.

It is the use by our modern progressive country of an ancient system of weights and measures which it is here proposed to discuss and show up as an absurdity. Our present system is organized and set forth in arithmetics under some fifteen so-called "tables." These tables are all different and there is no uniformity in any one table. Only one unit suggests convenience in reductions, viz., hundredweight. It is easy to reduce from pounds to hundredweight and *vice versa*. Some fifty ratio numbers have to be memorized or calculated from other memorized numbers to make the common needed reductions. History shows that ancient Babylonia had tables superior to those now in use, and ancient Britain a decimal scale which was crowded out by our present system.

The metric system of weights and measures was developed in France about 1800 and has come to be employed over all the civilized world except in the United States, Great Britain and Russia. The system was legalized in the United States in 1866 but not made mandatory and here we are fifty years later using the old system, with most of the civilized world looking on us with more or less scorn because of our belatedness.

In this age everywhere the cry is efficiency, always more efficiency. Ten thousand improvements and labor-saving devices are introduced every day. But here is an improvement and labor-saving device which would affect the life of every person in the land and in many instances greatly affect such persons' lives, and yet almost no one really knows anything about the matter.

So let us now consider the good points in the metric system (each implying corresponding elements of great weakness in the common system), and then study briefly what stands in the way of its adoption in this country. These good points are:

First, the metric units have uniform self-defining names (cent, mill,

meter and five more out of the eleven terms used already familiar to us in English words), are always the same in all lands, known everywhere, and fixed with scientific accuracy.

Second, every *reduction* is made almost instantaneously by merely moving the decimal point. There are no reductions performed by multiplying by 1,728 or 5,280, etc., or dividing by $5\frac{1}{2}$, $30\frac{1}{4}$ or $31\frac{1}{2}$, etc., and hence there is *a great saving* in the labor and time of making necessary calculations.

Third, there are but *five* tables in the metric system proper, these taking the place of from twelve to fifteen in our system (or lack of it). These are linear, square, cubic, capacity and weight.

Fourth, any one table is about as easy to learn as our United States money table, and after one is learned, it is much easier to learn the others, since the same prefixes with the same meanings are used in all.

Fifth, the weights of all objects are either known directly from their size, or can be very quickly found from their specific gravities.

Sixth, the subject is made so much easier for children in school that a conservative expert estimate of the saving is two thirds of a year in a child's school life. The rule in this country is eight years of arithmetic, the arithmetic occupying about one fourth of the child's activity. With metric arithmetic substituted for ours, what it now takes two years to prepare for, could be easily done in $1\frac{1}{3}$ years. This involves an enormous waste of money and energy every twelvemonth.

Seventh, only *one* set of measures and *one* set of weights are needed to measure and weigh everything, and *one* set of machines to make things for the world's use. There would be no duplication of costly machinery to enter the foreign trade field, thus securing enormous saving. It is well known that the United States and Great Britain have lost a vast amount of foreign commerce in competition with Germany and France, because of their non-use of the metric units. Britain realizes this and is greatly concerned over the situation.

Eighth, every ordinary practical problem can be solved conveniently on an adding machine. Our adding machines are used almost solely for United States money problems.

Ninth, no valuable time is lost in making reductions from common to metric units, or *vice versa,* either by ourselves or foreigners. To make our sizes in manufactured goods concrete to them foreign customers have to reduce our measures to theirs and this is a weariness to the flesh.

Tenth, the metric system is wonderfully simple. All the tables with a rule to make all possible reductions can be put on a postal card.[1]

The metric weights and measures constitute *a scientific system;* our weights and measures are a *disorganization.* Naturally one can expect a *great saving of time, thought and labor* from the use of a system, and

[1] See article by the writer in *Education* (Boston), Dec., 1894.

this is the fact. If one dared introduce ordinary arithmetical problems into an article like this, it would be easy to show by examples how a person has to be something of a master of common fractions in order to solve in our system common every-day problems, whereas in the metric system nearly everything is done very simply with decimals. In our system a mechanic after making a complicated calculation with common fractions is as likely as not to get his result in sixths, or ninths, etc., of an inch, whereas his rule reads to eighths, or sixteenths, and he must reduce his sixths, or ninths, to eighths, or sixteenths, before he can measure off his result. In the metric system results always come out in units of the scale used. The metric system measures to millimeters or to a unit a trifle larger than a thirty-second of an inch. In our system one is likely to avoid sixteenths or thirty-seconds on account of the labor of calculation. Then, besides, the amount of figuring is so much less in the metric system. Take the case of a certain problem to find the cubical contents of a box. Our solution calls for 80 figures and the metric for 35, and this is a typical case, not one specially selected. Thus, metric calculations, while only from one third to two thirds as long, are likely to be two or three times as accurate, are far easier to understand, and the results can be immediately measured off. Hence, we waste time in these four ways. Shakespeare in Hamlet says: "Thus conscience does make cowards of us all." In like vein it might be said: Thus custom (in weights and measures) doth make April fools of us all. It is no exaggeration to say that counting grown-ups solving actual problems and children solving problems in school we are sent on much more than a billion such April fool errands round Robin Hood's barn every year.

Noting how much time is saved in making simple every-day calculations by using the metric system, suppose that we assume of the 60 or more millions of adults in active life in this country, on the average only one in 60 makes such calculations daily and that only twenty minutes' time is saved each day. Let us suppose that the value of the time of the users is put at $2.40 per day or 10 cents for 20 minutes. Then 1,000,000 users would save $100,000 per day or $30,000,000 per year. But perhaps some one is saying that much of this time is not really saved, since many persons are paid for their time and can just as well do this work as not. The answer to this is that in many instances such calculations take the time of *others* as well as the person making the calculation. Occasionally a contractor might hold back, or work to a disadvantage a gang of a score of workmen while trying to solve a problem that came up unexpectedly.

An estimate of the value of all weighing and measuring instruments places the sum at $150,000,000. Thus, we see that in five years, merely by a saving in *time*—for time is money—all metric measuring and

weighing instruments could be got *new* at no extra expense. This estimate of the cost of replacing our weighing and measuring instruments by new metric ones and of saving time has been made by others with a similar result.

A matter of very much more importance than that just discussed is the extra unnecessary expense put upon education, viz., two thirds of a year for every child in the land. Presumably if the metric system were in use with us, all our children would stay in school as long as they now do, thus getting two thirds of a year farther along in the course of study. Actually, if arithmetic were made more simple, vast numbers would stay longer, since they would not be driven out of school by the terrible inroads on their interest in school work by dull and to them impossible arithmetic. If metric arithmetic texts were substituted for our present texts, it is safe to say children would average one full year more of education. What the increased earning power would be from this it would be hard to estimate, but clearly it would be a huge sum.

Consider also how much more life would be worth living for children, teachers and parents if a very large portion of arithmetical puzzles inserted to qualify the children to understand our crazy weights and measures were cut out of our text-books. If we were to adopt the metric system, literally millions of parents would be spared worry, and shame, and fear lest Johnny fail and drop out of school, or Mary show unexpected weakness and have to take a grade over again; uncounted thousands of teachers would be saved much gnashing of teeth and uttering of mild feminine imprecations under their breath; and, best of all, the children themselves would be saved from pencil-biting, tears, worries, heartburns, arrested development, shame and loss of education!

A committee of the National Educational Association has recently reported that Germany and France are each two full years ahead of us in educational achievement, that is, children in those countries of a certain age have as good an education as our children which are two years the foreign childrens' seniors. Surely one of these years is fully accounted for by the inferiority of our American *arithmetic* and *spelling*. This much, at least, of the difference is neither in the children themselves, nor in the lack of preparation of our teachers, nor in educational methods.

Professor J. W. A. Young, of the University of Chicago, in his work on "Mathematics in Prussia," says: "In the work in mathematics done in the nine years from the age of nine on, we Americans accomplish no more than the Prussians, while we give to the work seven fourths of the time the Germans give." Professor James Pierpont, of Yale, writing in the *Bulletin* of the American Mathematical Society (April, 1900), shows a like comparison can be made with French instruction. Pierpont's table exhibits only one hour a week needed for arithmetic for pupils

aged 11 and 12! As the advertisements sometimes say, there must be a reason.

But if the children are kept in school two thirds of a year longer somebody pays for this extra expense. Now children do not drop out of school until they are about 12 years of age and have both appetites and earning power. The number of these children that drop out each year is probably about 2½ millions. Of this number let us say 1½ millions would become wage earners, thus passing from the class that are supported to the class that support themselves and earn a small wage besides. We have then three items in this count: (1) The cost to the state in taxes for the education of 2½ million for two thirds of a year, or $50,000,000; (2) The cost to the parents for support of 1½ millions for two thirds of a year at $67 each, or $100,000,000; (3) The wages of 1½ millions over and above the cost of their support, say $50 each, or $75,000,000.

The above figures are put low purposely so that they can not be criticized. It should be remembered that 46 per cent. of our population is agricultural, and that on the farm, youths of from 13 to 15 very often do men's and women's work: also that in many manufacturing centers great numbers of children get work at relatively good wages, and that the number of completely idle children out of school is not large.

With these figures in hand let us consider now a kind of debit and credit sheet against and for our present system of weights and measures.

PRESENT SYSTEM OF WEIGHTS AND MEASURES

In *annual* account with UNCLE SAM

Dr.		Cr.
To cost in school taxes of keeping 2½ millions of children in school ⅔ year.	$50,000,000	By culture (?) acquired by the children through learning more common fractions and our crazy tables of weights and measures ....$?
To cost to parents for supporting 1½ millions children ⅔ year	100,000,000	
To loss of productive power of 1½ millions youth for ⅔ year	75,000,000	
To loss of earning power by having children driven out of school by difficulties of arithmetic as now taught	25,000,000	
To loss of time in making arithmetical calculations by men in trade, industries and manufactures	30,000,000	
To extra weighing and measuring instruments needed for sundry tables.	10,000,000	
To loss of time in making cross reductions to and from our system and metric system	5,000,000	
To loss of profit from foreign trade because our goods are not in metric units	20,000,000	
Total annual loss	$315,000,000	

Commenting for a moment on the credit side of the above ledger account, it can be said that recent psychology shows conclusively that training in common fractions and weights and measures can not be of much practical help as so-called culture, or training for learning other things, unless those other things are closely related to them, and there are not many things in life so related to them once we had dropped our present weights and measures.

It may be complained that the expense of changing to the new system is not taken account of in the above table. The reason is that that expense would occur once for all. The above table deals with the *annual* cost of our present medieval system.

One powerful reason for the adoption of the metric system different in character from the others is the ease of cheating by the old system. In the past the people have been unmercifully abused through short weights and measures. Many of the states have taken this matter up latterly and prosecuted merchants right and left. Nine tenths of this trouble would disappear with the new system in use.

Let us consider now for a little time the reasons why the metric system has not been accepted and adopted for use in the United States. Evidently the great main reason has been that the masses of the people, in fact all of them except a very small educated class in science are almost totally uninformed on this whole question. Such articles as have been published have almost invariably appeared in either scientific, technical or educational magazines, mostly the first, so that there has been no means of reaching the masses, or even the school teachers with the facts. For another reason the United States occupies an isolated position geographically, and our people do not come into personal contact with those in other countries using the metric system. But there is still another potent reason. After the United States government legalized the metric system in 1866, all the school books on arithmetic began presenting the topic of the metric system, and, quite naturally, they did it by comparing its units with those of our system and calling for cross reductions from one system to the other. No better means of sickening the American children with the metric system could have been devised. Multitudes of the young formed a strong dislike for the foreign system with its foreign names, and could not now be easily convinced that it is not difficult to learn. Every school boy knows how easy it is to learn United States money. The boy just naturally learns it between two nights. The whole metric system *under favorable conditions* is learned nearly as easily. By favorable conditions is meant the constant use of the system in homes, schools, stores, etc. These favorable conditions, of course, we have never had.

In 1904 an earnest effort was made again both in this country and

Great Britain to have the metric system adopted for general use. The exporting manufacturers in both countries grew much concerned over the whole situation. A petition to have the metric system adopted in Great Britain was signed by over 2,000,000 persons. A bill to make the system mandatory was passed by the House of Lords and its first reading in the House of Commons. The forces of conservatism then bestirred themselves and the bill was held up. Forseeing a movement of the same kind in this country, the American Manufactures' Association got busy, laid plans to defeat such movement which they later did. Strictly speaking this action was not taken by the association as such but only by a part of it. One fourth of the membership and probably much more than a fourth of the capital of the association was on the side for the adoption of the system. Politically, however, the side opposed to the new system had altogether the most influence.

Careful study of the whole matter showed that the main cost to make the change to the new system would be in dies, patterns, gauges, jigs, etc. A careful estimate put this cost at $600 for each workman and assuming a million workmen, we have a total cost of $600,000,000. But we have just seen that the annual expense of retaining the old system of weights and measures is over $300,000,000. Thus we see that two short years would suffice to pay for what seems to the great manufacturers association an insuperable expense. From all this we see that the question is not one for N. M. A. bookkeeping, but for national bookkeeping.

Many well-informed people studying the matter superficially, think the difficulties in the way of a change to the new system insurmountable. Thus, they think of the cost to the manufacturer—which we have just seen to be rather large but not insurmountable; they think of the changes needed in books, records, such as deeds, and the substitution of new measuring and weighing instruments. Germany and all the other countries of continental Europe made the change. Are we to assume that the United States can not? That would be ridiculous. Granting that commerce has grown greatly; so also has intelligence and capability of the people for doing great things.

Scientists are universally agreed as to the wisdom of the adoption of the metric system. The country, as a whole, must be educated up to the notion that sooner or later it is sure to be universally adopted, that it is only a question of time when this will be done. Already electrical, chemical and optical manufacturing concerns use the metric units and system exclusively. The system is also used widely in medicine and still other arts. Then all institutions of learning use the metric system exclusively whenever this is possible. All that is needed is to complete a good work well begun.

There is one rational objection to the metric system and but one.

It is that 10 is inferior to 12 as a base for a notation for numbers, but the world is not ready to make this change nor is it likely to be for generations to come. Moreover, this improvement is far less important than uniformity in weights and measures. For these reasons this objection can be passed over. Men said the metric system would never be used outside of France; but it has come to be used all over the world. The prophets said we should never have uniformity as regards a reference meridian of longitude. But we have. And so it will be with the adoption of the metric system in the United States and Great Britain. It is only a question of whether it comes sooner or later. When that day comes, the meter, a long yard, will replace the yard, the liter, the quart (being smaller than a dry and larger than a liquid quart), the kilogram will replace the pound, being equal to 2.2 pounds, and the kilometer (.6 mi.) will replace the mile. Within a week or so after the change has been made to the new system, all men in business will be reasonably familiar with the new units and how they are used, and within a few months every man, woman and child will be as familiar with the new system as they ever were with the simplest parts of the old. So easy it will be to make the change as far as ordinary business affairs are concerned. However, for exact metal manufactures years will be needed to fully change over to the new. Here the plan is to begin with new unit constructions and new models, as automobiles using new machinery constructed in the integral units of the metric system. All old constructions are left as they are and repaired as they are. This was the plan used in Germany and of course it works.

In conclusion it can be said that we started with the idea that the change to the metric system was needed for the sake of foreign commerce. We now see that we need it also for our own commercial and manufacturing transactions. If we are to have the efficiency so insistently demanded by the age in which we live, then we must have the metric system in use for the ordinary affairs of daily life of the masses of the people, we must have it in commercial and manufacturing industries, and we must have it in education. If efficiency is to be the slogan, then the metric system must come no matter what obstacles stand in its way.

ADAPTATION AS A PROCESS

By Professor HARRY BEAL TORREY
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FOR the physicist and chemist the term adaptation awakens but the barren echo of an idea. In biology it still retains a certain standing, though its significance has, in recent years, been rapidly contracting, as the influence of the conception for which it stands has waned. Many biologists are now of the opinion that their science would be better off entirely without it. They believe it has not only outlived its usefulness, but has become a source of confusion, if not, indeed, reaction.

Darwin's first task, in the "Origin of Species," was to demonstrate that species had not been independently created, but had descended, like varieties, from other species. But he was well aware that

> such a conclusion, even if well founded, would be unsatisfactory until it could be shown how the innumerable species inhabiting the world have been modified, so as to acquire that perfection of structure and coadaptation which justly excites our admiration.

To establish convincingly the doctrine of descent with modification as a theory of species, it was necessary for him to develop the theory of adaptation which we now know as natural selection.

The origin of adaptive variations gave him, at that time, little concern. Though keenly appreciative of the problem of variation which his studies in evolution presented, he dismissed it in the "Origin" with less than twenty-five pages of discussion. Such brevity is not surprising, since a more extended treatment would only have embarrassed the progress of the argument. In fact, his restraint in this direction enabled him, first, to avoid the difficulties into which Lamarck, with his bold attack on the problem of variation, had fallen; and second, by doing so, to deal the doctrine of Design a blow from which it has never recovered.

The latter was a service of well-nigh incalculable value to the young science of biology—and, as it appeared, to modern civilization as well. But it has not been uncommon, from Aristotle's day to this, for the work of great men to suffer at the hands of less imaginative followers. Sweeping applications of Darwin's doctrine have been repeatedly made without due regard either for its original object or for the success with which that object was achieved. So I believe it to be no fault of Darwin that the growing indifference of European laboratories toward natural selection should find occasional expression in such a phrase as

"the English disease." Disease, indeed, I believe we must in candor admit that devotion to it to be which blinds its devotees to those problems of more elementary importance than the problem of adaptation, which Darwin clearly saw but was born too soon to solve.

The problem of species has profoundly changed since 1859. For Darwin it was perforce a problem of adaptation. For the investigator of to-day it has become a part of the more inclusive problem of variation. Along with the logical results of natural selection he contemplates the biological processes of organic differentiation. He is no longer satisfied to assume the existence of those modifications that make selection possible. In his efforts to control them, the conception of adaptation as a result has been crowded from the center of his interest by the conception of adaptation as a process.

The survival of specially endowed organisms, the elimination of competing individuals not thus endowed, are facts that possess, in themselves, no immediate biological significance. Selection as such is not a biological process, whether it is accomplished automatically on the basis of protective coloration, or self-consciously by man. Separating sheep from goats may have a purely commercial interest, as when prunes and apples, gravel and bullets, are graded for the market. Such selection is, at bottom, a method of classification, serving the same general purpose as boxes in a post-office. Similarly, natural selection is but a name for the segregation and classification that take place automatically in the great struggle for existence in nature. The fact that it is a result rather than a process accounts, probably more than anything else, for its remarkable effect upon modern thought. It is non-energetic. It exerts no creative force. As a conception of passive mechanical segregation and survival, it was a most timely and potent substitute for the naïve teleology involved in the idea of special creation.

As a theory of adaptation, then, natural selection is satisfactory only in so far as it accounts for the "*preservation* of favored races." It throws no light upon the origin of the variations with which races are favored. Since it is only as variations possess a certain utility for the organism that they become known as adaptations, the conception of adaptation is inevitably associated with the welfare of individuals or the survival of races. To disregard this association is to rob the conception of all meaning. Like health, it has no elementary physiological significance.

Our profound interest in the problem of survival is natural and practical and inevitable. But in spite of Darwin's great contribution toward a scientific analysis of the mechanism of organic evolution, and in spite of the marvelous recent progress of medicine along its many branches, the fact remains that so far as this interest in the problem of survival is dominant it must continue to hinder adequate analysis

of the problem of adaptation. Indeed, it is in large measure due to such domination in the past that biology now lags so far behind the less personal sciences of physics and chemistry. For survival means the survival of an individual. And there is no doubt that the individual organism is the most conspicuous datum in the living world. The few who, neglectful of individuals and survivals, find their chief interest in living substance, its properties and processes, are promptly challenged by the many to find living substance save in the body of an organism. Thus, in a peculiarly significant sense, organisms are vital units. And since the individual organism shows a remarkable capacity to retain its identity under a wide range of conditions, adaptability or adjustability comes to be reckoned as the prime characteristic of life by all to whom the integrity of the individual organism is the fact of chief importance.

With the use of the words adaptability and adjustability, our discussion assumes a somewhat different aspect. Instead of contemplating further the mechanical selection of individuals on the basis of characters that, like the structure of "the woodpecker, with its feet, tail, beak and tongue, so admirably adapted to catch insects under the bark of trees," can not be attributed to the influence of the external conditions that render them useful, we are invited to consider immediate and plastic adjustments of the organism to the very conditions that call forth the response. For the fortuitous adjustments that tend to preserve those individuals or races that chance to possess them, are substituted, accordingly, the direct primary adjustments that tend to preserve the identity of the reacting organism. We turn thus from the *results* of the selection of favorable variations to the biological *processes* by which organisms become accommodated to their conditions of life.

At once the old questions arise. Are these processes fundamentally peculiar to the life of organisms? Does the capacity of the organism thus to adjust itself to its environment involve factors not found in the operations of inorganic nature? Our answers will be determined essentially by the nature of our interest in the organism—whether we regard its existence as the *end* or merely an incidental *effect* of its activities. The first alternative is compatible with thoroughgoing vitalism. The second, emphasizing the nature of the processes rather than their usefulness to the organism, relieves biology of the embarrassments of vitalistic speculation, and allies it at the same time more intimately than ever with physics and chemistry. This alliance promises so well for the analysis of adaptations, as to demand our serious attention.

Physiologically, the living organism may be thought of as a physicochemical system of great complexity and peculiar composition which varies from organism to organism and from part to part. Life itself

may be defined as a group of characteristic activities dependent upon the transformations in this system under appropriate conditions. According to this definition, life is determined not only by the physical and chemical attributes of the system, but by the fitness of its environment, which Henderson has recently done the important service of emphasizing.[1] Relatively trifling changes in the environment suffice to render it unfit, however, that is, to modify it beyond the limits of an organism's adaptability. The environmental limits are narrow, then, within which the transformations of the organic system can take place that are associated with adaptive reactions. The conditions within these limits are, further, peculiarly favorable for just such transformations in just such physico-chemical systems.

The essential characteristic of the adaptive reaction appears to be that the organism concerned responds to changing conditions without losing certain attributes of behavior by which we recognize organisms in general and by which that organism is recognized in particular. It exhibits stability in the midst of change; it retains its identity. But this stability, let us repeat, is the stability of a certain type of physico-chemical system, with respect to certain characters only, and exhibited under certain circumscribed conditions. In so far as the problem of adaptation is thus restricted in its application, it remains a question of standards, a taxonomic convenience, a problem of the organism by definition only, empty of fundamental significance.

It is to be expected that systems differing widely in composition and structure will differ in their responses to given conditions. This will be true whether the systems compared thus are organic, or inorganic, or representative of both groups. The compounds of carbon, of which living substance is so characteristically composed, exhibit properties and reactions that distinguish them at once in many respects from the compounds of lead or sulphur. They also differ widely among themselves; compare, in this connection, serum albumen, acetic acid, cane sugar, urea. No vitalistic factor is needed for the interpretation of divergencies of this kind. But there are many significant similarities between organisms and inorganic systems as well. These are so frequently overlooked that it will now be desirable to consider a few illustrative cases. For the sake of brevity, they have been selected as representative of but two types of adaptation commonly known under the names of *acclimatization* and *regulation*.

Let us first consider the case of organisms which become acclimatized by slow degrees to new conditions that, suddenly imposed, would produce fatal results. *Hydra* is an organism which becomes thus acclimatized finally to solutions of strychnine too strong to be endured at first. Outwardly it appears to suffer in the process no obvious modi-

[1] "The Fitness of the Environment."

fications. Yet modifications of a physiological order take place, as is shown, first, by the necessary deliberation of the acclimatization, second, by the death of the organism if transferred abruptly back to its original environment.

In other forms the structural changes accompanying acclimatization may be far more conspicuous. For example, the aerial leaves of *Limnophila heterophylla* are dentate, while those grown under water are excessively divided. Again, the helmets and caudal spines of *Hyalodaphnia* vary greatly in length with the seasonal temperature.

In these and the large number of similar cases that might be cited, stability of the physiological system under changed conditions is only obtained by changes in the system itself which are often exhibited by striking structural modifications.

Compare with such phenomena of acclimatization the responses of sulphur, tin, liquid crystals and iron alloys to changes of temperature. The rhombic crystals that characterize sulphur at ordinary temperatures and pressures, give place to monoclinic crystals at 95.5° C. Sulphur thus exists with two crystalline forms whose stability depends directly upon the temperature.

Similarly, tin exists under two stable forms, white and gray, the one above, the other below the transitional point, which is, in this case, 18° C. At this temperature white tin is in a metastable condition, and transforms into the gray variety. The transformation goes on, then, at ordinary temperatures, but, fortunately for us as users of tin implements, very slowly. Its velocity can be increased, however, by lowering the temperature, on which, then, not only the transformation itself, but its rate depends.

In this connection may be mentioned cholesteryl acetate and benzoate and other substances which possess two crystalline phases, one of which is liquid, unlike other liquids, however, in being anisotropic. As in the preceding cases, these phases are expressions of equilibrium at different temperatures.

Especially instructive facts are afforded by the alloys of iron and carbon. Iron, or ferrite, exists under three forms: as α ferrite below 760°, as β ferrite between 760° and 900°, and as γ ferrite above 900°. Only the last is able to hold carbon in solid solution. The alloys of iron and carbon exist under several forms. *Pearlite* is a heterogeneous mixture containing 0.8 per cent. carbon. When heated to 670°, it becomes homogeneous, an amount of carbon up to two per cent. dissolves in the iron, and hard steel or *martensite* is formed. In appearance, however, the two forms are so nearly identical as to be discriminated only by careful microscopical examination. *Cementite* is a definite compound of iron and carbon represented by the formula Fe_3C.

When cooled slowly below 670°, *martensite* yields a heterogeneous mixture of *pearlite* and *ferrite* (or *cementite*, if the original mixture

contained between 0.8 per cent. and two per cent. of carbon). Soft steels and wrought iron are thus obtained. When cooled rapidly, however, as in the tempering of steel, martensite remains a homogeneous solid solution, or hard steel.

One can not fail to notice the remarkable parallel between these facts and the behavior of *Hydra* in the presence of strychnine. In both cases new positions of stability are reached by modifying the original conditions of stability; and in both, the old positions of stability are regained only by returns to the original conditions of stability so gradual as to afford time sufficient for the necessary transformations in the systems themselves.

The forms which both organic and inorganic systems assume thus appear to be functions of the conditions in which they exist.

The fact that *Hydra* is able to regain a position of stability from which it had been displaced connects the behavior of this organism not only with the physical phenomena already cited, but still more intimately with the large class of chemical reactions which are similarly characterized by equilibrium and reversibility. Such reactions do not proceed to completion, which is probably always the case wherever the mixture of the systems under transformation is homogeneous, as in the case of solutions. They occur widely among carbon compounds. The following typical case will suffice to indicate their essential characteristics.

When ethyl alcohol and acetic acid are mixed, a reaction ensues which yields ethyl acetate and water. But ethyl acetate and water react together also, yielding ethyl alcohol and acetic acid. This second reaction, in a direction opposite to the first, proceeds in the beginning more slowly also. There comes a time, however, when the speeds of the two reactions are equal. A position of equilibrium or apparent rest is thus reached, which persists as long as the relative proportions of the component substances remain unchanged.

A great many reversible reactions are made possible by enzymes. In the presence of diastase, glucose yields glycogen and water, which, reacting together in the opposite direction, yield glucose again. In the presence of emulsin, amygdalin is decomposed into glucose, hydrocyanic acid and benzoic aldehyde, and reformed from them. Similarly in the presence of lipase, esters are reformed from alcohols and fatty acids, their decomposition products.

With the introduction of enzymes, certain complications ensue. Though it has been shown that lipase acts as a true catalyser, this may not hold for all, especially for proteolytic, enzymes. That reversible reactions actually occur in proteids, however, accompanied as they are in some cases at least by certain displacements of the position of equilibrium, there appears to be no question.[2]

[2] Robertson, Univ. Calif. Publ. Physiol., 3, 1909, p. 115.

These examples are but suggestions of the many reversible reactions that have now been observed among the compounds of carbon. That they have peculiar significance for the present discussion resides in the fact that living substance is composed of carbon compounds, so many and in such exceedingly complex relations as to present endless possibilities for shifting equilibria and the physical and chemical adjustments resulting therefrom.

With these facts in mind we may now turn from the consideration of acclimatization to a brief discussion of certain phenomena of regulation—adaptive reactions that are especially conspicuous in the growth and development of organisms, but separated by no sharp dividing line from adaptive reactions of the other type.

When a fragment of an organism transforms, under appropriate conditions, into a typical individual, the process includes degenerative as well as regenerative phases. There is always some simplification of the structures present, whose character and amount is determined by the degree of specialization which has been attained. The smaller the piece, within certain limits, and the younger physiologically, the more nearly does it return to embryonic conditions, a fact which can be studied admirably in the hydroid *Corymorpha*. In some cases the simplification is accomplished by abrupt sacrifice of highly specialized parts, as in *Corymorpha,* when in a process of simplification connected with acclimatization to aquarium conditions, the large tentacles of well-grown specimens fall away completely from their bases. In other hydroids (*e. g., Campanularia*) the tentacles may be completely absorbed into the body of the hydranth from which they originally sprang. Among tissue cells degenerative changes may be abrupt, as in the sacrifice of the highly specialized fibrillæ in muscle cells; or they may be very gradual, as in the transformation of cells of one sort into another that occurs in the regeneration of tentacles in *Tubularia.*

An interesting case of absorption of parts came to my notice while studying the larvæ of the pennatulid coral *Renilla* some fifteen years ago. As will be remembered, *Renilla* possesses eight tentacles with numerous processes pinnately arranged. During a period of enforced starvation, these pinnæ were gradually absorbed, and the tentacles shortened, from tip to base. With the advent of food—in the form of annelid eggs—the reverse of these events took place. The tentacles lengthened and the pinnæ reappeared, the larvæ assuming their normal aspect.

It appears, then, that in some circumstances at least, the process of simplification may resemble very nearly, even in details, a reversal of the process of differentiation. That one is actually in every respect the reverse of the other is undoubtedly not true. This, however, is not to be wondered at. Mechanical inhibitions that are so conspicuous in

some cases (*e. g., Corymorpha*) are to be expected to a certain degree in all. The regenerative process itself depends upon the cooperation of many physical and chemical factors, in many and complex physico-chemical systems in varying conditions of equilibrium. And it is important to note that even the equilibrium reactions by which a single proteid in the presence of an enzyme, is made and unmade, do not appear always to follow identically the same path in opposite directions.[3]

Whatever their course in the instances cited and in many others, reversals in the processes of development do take place. In perhaps their simplest form these can be seen in egg cells. The development of a fragment of an egg as a complete whole involves reversals in the processes of differentiation of a very subtle order. The fusion of two eggs to one involves similar readjustments. Such phenomena have been held to be peculiar to living machines only. Yet it may be pointed out that there are counterparts of both in the behavior of so-called liquid crystals. When liquid crystals of paraazoxyzimtsäure-Äthylester are divided, the parts are smaller in size, but otherwise identical with the parent crystal in form, structure and optical properties. The fusion of two crystals of ammonium oleate forming a single crystal of larger size has also been observed. Though changes in equilibrium that accompany such behavior of liquid crystals are undoubtedly very much simpler than the changes that accompany the regulatory processes exhibited by the living egg, the striking resemblance between the phenomena themselves tempts us not to magnify the difference.

Further temptation in the same direction is offered by the recent discovery[4] that the processes of development stimulated in the eggs of the sea urchin *Arbacia* by butyric acid or weak bases, and evidenced by the formation of the fertilization membrane, is reversible. When such eggs are treated with a weak solution of sodium cyanide or chloral hydrate, they return to the resting condition. Upon fertilization with spermatozoa, in normal sea water, they proceed again to develop.

The facts that have now been briefly summarized have been selected to emphasize the growing intimacy between the biological and the inorganic sciences. No harm can conceivably come from it. On the contrary, there is every reason to be hopeful that the investigation of biological problems in the impersonal spirit that has long distinguished the maturer sciences of physics and chemistry will continue to develop a better control and fuller understanding of the processes in living organisms, of which the phenomena of variation in general, and of adaptation in particular, are but incidental effects.

[3] Robertson, *vid. sup.*, p. 269.

[4] Loeb, *Arch. f. Entw.*, 28, 1914, p. 277.

WHY CERTAIN PLANTS ARE ACRID

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EVER since my first lessons in botany, the characteristic qualities and properties of plants have given me much thought. Why certain plants produced aromatic oils and ethers, while others growing under the same conditions produced special acids or alkaloids, was a subject of endless speculation.

The pleasing aroma of the bark of various trees and shrubs, the spicy qualities of the foliage and seeds of other plants; the intense acridity; the bitterness; the narcotic, the poisonous principle in woody and herbaceous species; all were intensely interesting.

This interest was biological rather than chemical. I cared less for the ultimate composition of the oils, acids, alkalis, etc., than I did for their use or office in the plant economy, and their effect upon those who might use them.

Perhaps no one plant interested me more from this point of view, than the well-known Indian turnip (*Arisæma triphyllum*). As a boy I was well acquainted with the signally acrid quality of this plant; I was well aware of its effect when chewed, yet I was irresistibly drawn to taste it again and again. It was ever a painful experience, and I suffered the full penalty of my rashness. As an awn from a bearded head of barley will win its disputed way up one's sleeve, and gain a point in advance despite all effort to stop or expel it, so did every resolution, every reflection, counteract the very purpose it was summoned to oppose, and to my sorrow I would taste the drastic, turnip-shaped corm wherever opportunity occurred.

It is a well-known fact that the liquid content of the cells of plants contain numerous inorganic substances in solution. Among these, not considering oxygen, hydrogen, nitrogen and carbon dioxide, there are the salts of calcium, magnesium, potassium, iron, sulphur and phosphorus. The above substances are found in the cells of every living plant. Other substances like salts of sodium and silica are also found, but these are not regarded as essential to the life and growth of plants. They appear to be present because the plant has not the power to reject them. Many of the substances named above, are found deposited either in an amorphous or crystalline form in the substance of the cell wall. In addition to this, crystals of mineral matter, having various shapes and sizes, are often found in the interior of cells. The most com-

mon of these interior cell crystals are those composed of calcium oxalate and calcium carbonate. Others composed of calcium phosphate, calcium sulphate and silica are sometimes found. These crystals may occur singly or in clusters of greater or less size. In shape they are prismatic or needle-like.

It is not the object of this paper to treat of plant crystals in general, but to consider the peculiar effect produced by certain forms when found in some well-known plants.

The extreme acridity or intense pungency of the bulbs, stems, leaves and fruit of various species of the Araceæ or Arum family, was recognized centuries ago. The cause of this characteristic property or quality was, until a comparatively recent date, not definitely determined.

As far as I am aware the first scientific investigation of this subject was made by the writer. At a meeting of the American Association for the Advancement of Science held at Indianapolis in 1890, some studies and experiments were reported in a short paper entitled "Notes upon the Crystals in certain species of the Arum Family."

This paper expressed the belief that the acridity of the Indian turnip and other plants belonging to the same family, was due to the presence of needle-shaped crystals or raphides found in the cells of these plants. This conclusion was not accepted by Professor T. J. Burrill, of the University of Illinois, nor by other eminent botanists who were present and took part in the discussion that followed the reading of the paper.

The opposition was based mainly on the well-known fact that many other plants like the grape, rhubarb, fuchsia, spiderwort, etc., are not at all, or but slightly acrid, although the raphides are as abundant in them as in the Indian turnip and its allies.

Up to this time the United States Dispensatory and other works on pharmacy, ascribed the following rather indefinite cause for the acridity of the Indian turnip. It was said to be due to an acrid, extremely volatile principle. This principle was insoluble in water and alcohol, but soluble in ether. It was dissipated both by heating and drying, and by this means the acridity is destroyed. There was no opinion given as to the real nature of this so-called principle.

More recently it has been intimated that the acridity may be due to some ferment or enzyme, which has been derived in part from the self-decomposition of protoplasm and in part by the process of oxidation and reduction.

Here the question appeared to rest. At all events I was unable to glean any further knowledge from the sources at my command.

Some time later the subject was taken up in a more comprehensive manner and the following report is the first detailed description of an

investigation that has occupied more or less of my leisure for some years.

A dozen or more species of plants have been used for examination and study. Among these were:

Indian turnip (*Arisæma triphyllum*).
Green dragon (*Arisæma dracontium*).
Sweet-flag (*Acorus*).
Skunk cabbage (*Spathyema*).
Calla (*Richardia*).
Caladium (*Caladium*).
Calocasia (*Calocasia*).
Phyllodendron (*Phyllodendron*).
Fuchsia (*Fuchsia*).
Wandering Jew (*Tradescantia*).
Rhubarb (*Rheum*).
Grape (*Vitis*).
Onion (*Allium*).
Horse-radish (*Armoracia*).

Most of the plants selected were known to have crystals in certain parts. Some of them were known to be intensely acrid. In these the acridity was in every instance proportional to the number of crystals.

The following order of study was pursued and the results of each step noted. Only the more salient points of the methods employed and the conclusions reached are presented.

1. *The Character of the Taste Itself.*—It was readily noted that the sensation produced by chewing the various acrid plants was quite different. For example, the Indian turnip and its close allies do not give the immediate taste or effect that follows a similar testing of the onion or horse-radish. When the acridity of the former is perceived the sensation is more prickling than acrid.

The effect produced is more like the pricking of numerous needles. It is felt not only upon the tongue and palate, but wherever the part tasted comes into contact with the lips, roof of mouth or any delicate membrane. It is not perceived where this contact does not occur.

The acridity of the onion and horse-radish is perceived at once and often affects other parts than those with which it comes into direct contact.

2. *The Acrid Principle Is Not Always Volatile.*—This is shown by the fact that large quantities of the mashed or finely grated corms of the Indian turnip and allied species, produced no irritation of the eyes or nose even when these organs were brought into close contact with the freshly pulverized material. This certainly is in marked contrast with the effect produced by freshly grated horse-radish, peeled onions, crushed mustard seed when the same test is applied.

It seems fair to assume that in the latter case some principle that

is volatile at ordinary air temperatures is present. The assumption that such principle is present in the former has no room.

In order to test this matter further a considerable quantity of the juice of the Indian turnip was subjected to careful distillation, with the result that no volatile principle or substance of any kind was found.

Various extractive processes were tried by using hot and cold water, alcohol, chloroform, benzene, etc. These failed in every instance to remove any substance that had a taste or effect anything like that found in the fresh Indian turnip.

3. *The Acrid Principle Is Not Soluble in Ether.*—Inasmuch as various works on pharmacy made the claim that the active or acrid principle of the plants in question was soluble in ether, this was the next subject for investigation. The juice was expressed from a considerable quantity of the mashed Indian turnip. This juice was clear and by test was found to possess the same acrid property as the unmashed corms.

Some of the juice and an equal quantity of ether were placed into a cylinder and well shaken. After waiting until the ether had separated a few drops of the liquid were put into the mouth. For a little time no result was perceived, but as soon as the effect of the ether had passed away the same painful acridity was manifest as was experienced before the treatment with the ether. A natural conclusion from this test was that the acridity might come from some principle soluble in ether.

Observing that the ether was quite turbid and wishing to learn the cause, a drop or two was allowed to evaporate on a glass slide. Examining the residue with a microscope it was found to consist of innumerable raphides or needle-like crystals. Some of the ether was then run through a filter. The filtrate was clear. An examination showed it to be entirely free from raphides, and it had lost every trace of its acridity. The untreated acrid juice of the Indian turnip, calla, and other plants of the same family was then filtered and in every instance the filtered juice was bland and had lost every trace of its acridity. These tests and others that need not be mentioned, proved conclusively that the acridity of various species of the Arum family was not due to a volatile principle, but was due to the needle-shaped crystals found so abundantly in these plants.

Several questions yet remained to be answered. (1) If these needle-like crystals or raphides are the cause of the acridity of the plants just mentioned, why do they not produce the same effect in the fuchsia, tradescantia and other plants where they are known to be just as abundant? (2) Why does the Indian turnip lose its acridity on being heated? (3) Why does the dried Indian turnip lose its acridity?

It was first thought that the raphides found in plants having no

acridity, might be of different chemical composition than those which produce this effect.

A chemical examination proved beyond question that the raphides were of the same composition. The needle-shaped crystals in all the plants selected for study were composed of calcium oxalate. The crystals, found in grape, rhubarb, fuchsia and tradescantia were identical in form, fineness and chemical composition with those found in the plants of the Arum family. How then account for the painfully striking effect in one case and the non-effect in the other? This was the perplexing question.

In expressing some juice from the stems and leaves of the fuchsia and tradescantia it was found to be quite unlike that of the Indian turnip and calla. The juice of the latter was clear and limpid; that of the former quite thick and mucilaginous. There was no difference as to the abundance of crystals revealed by the microscope.

After diluting the ropy, mucilaginous juice with water, and shaking it thoroughly with an equal volume of ether, there was no turbidity seen in the supernatent ether. Allowing a few drops of the ether to evaporate scarcely any crystals could be found. Practically none of them had been removed from the insoluble mucilaginous covering. Here and there an isolated specimen was all that could be seen. So closely were these small crystals enveloped with the mucilaginous matter that it was almost impossible to separate or dissect them from it.

It was now easy to explain why certain plants whose cells were crowded with raphides were bland to the taste, while other plants with the same crystals were extremely acrid.

In one case the crystals were neither covered nor embedded in an insoluble mucilage, but were free to move. Thus when the plant was chewed or tasted the sharp points of these needle-like crystals came into contact with the tongue, lips and membranous surface of the mouth.

In the other case the insoluble mucilage which surrounded the crystals prevented all free movement and they produced no irritation.

Why do these intensely acrid, aroid plants lose their acridity on being heated? It is well known that the corms of the Indian turnip and its allies contain a large amount of starch. In subjecting this starch to heat it becomes paste-like in character. This starch paste acts in the same manner as the insoluble mucilage. It prevents the free movement of the crystals and in this way all irritant action is precluded. In heating the Indian turnip and other corms, it was found that the heat applied must be sufficient to change the character of the starch or the so-called acridity was not destroyed.

One other question remains to be answered. It has long been noted that the old or thoroughly dried corms of the Indian turnip are not acrid like those that are fresh. The explanation is simple. As the

plant dries or loses its moisture, the walls of the cells collapse and the crystals are closely encased in the hard, rigid matter that surrounds them. This prevents free movement and the crystals can not exert any irritant action.

It is generally believed by biologists that the milky juice, aromatic compounds, alkaloids, etc., found in plants have no direct use in the economy of the plant. They are not connected with the nutritive processes. They are excretions or waste products that the plant has little or no power to throw off. There can be little doubt, however, that these excretory substances often serve as a means of protection. Entomologists have frequently stated that the milky juice and resins found in the stems of various plants act as a protection against stem boring insects. In like manner the bulbs, stems and leaves of plants that are crowded with crystals have a greater immunity from injurious biting insects than plants that are free from crystals. It is quite generally believed that the formation of crystals is a means of eliminating injurious substances from the living part of the plant. These substances may be regarded as remotely analogous to those organic products made by man in the chemical laboratory.

Some progress has been made in this direction, but so far the main results are certain degradation-products such as aniline dyes derived from coal tar; salicylic acid; essences of fruits; etc. Still these and many other discoveries of the same nature do not prove that the laboratory of man can compete with the laboratory of the living plant cell.

Man has the power to break down and simplify complex substances and by so doing produce useful products that will serve his purposes. We may combine and re-combine but so far we only replace more complex by simpler combinations.

The plant alone through its individual cells, and by its living protoplasm has fundamentally creative power. It can build up and restore better than it can eliminate waste products.

HOW OUR ANCESTORS WERE CURED

By Professor CARL HOLLIDAY
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SUPPOSE you had a bad case of rheumatism, and your physician came to your bedside and exclaimed loudly, "Hocus pocus, toutus talonteus, vade celeriter jubeo! You are cured." What would you think, what would you do, and what fee would you pay him? Probably, in spite of your aches and pangs, you would make astonishing speed—for a rheumatic person—in proffering him the entire room to himself. But there was a time—and that as late as Shakespeare's day—when so-called doctors in rural England used just such words not only for rheumatism, but for many another disease. And to this hour the fakir on the street corner uses that opening expression, "Hocus pocus." Those words simply prove how slowly the Christian religion was absorbed by ancient Anglo-Saxon paganism; for "Hocus pocus" is but the hastily mumbled syllables of the Catholic priest to his early English congregation—"Hoc est corpus," "this is the body"; and the whole expression used by the old-time doctor meant merely that in the name of the body of Christ he commanded the disease to depart quickly.

How superstitions and ancient rites do persist. To this hour the mountaineers of southwestern Virginia and eastern Tennessee believe that an iron ring on the third finger of the left hand will drive away rheumatism, and to my personal knowledge one fairly intelligent Virginian believed this so devoutly that he actually never suffered with rheumatic pains unless he took off the iron ring he had worn for fifteen years. It is an old, old idea—this faith in the ring-finger. The Egyptians believed that a nerve led straight from it to the heart; the Greeks and Romans held that a blood-vessel called the "vein of love" connected it closely with that organ; and the medieval alchemists always stirred their dangerous mixtures with that finger because, in their belief, it would most quickly indicate the presence of poison. So, too, many an ancient declared that whenever the ring-finger of a sufferer became numb, death was near at hand. Thus in twentieth century civilization we hear echoes of the life that Rameses knew when the Pyramids were building.

Our Anglo-Saxon forefathers had great faith in mysterious words. The less they understood these the more they believed in the curative power. Thus the name of foreign idols and gods brought terror to the local demons that enter one's body, and when Christianity first

entered England, and its meanings were but dimly understood, the names of saints, apostles and even the Latin and Greek forms of "God" and "Jesus" were enemies to all germs. Then, too, what comfort a jumbling of many languages brought to the patient, especially if the polyglot cure were expressed in rhythmic lines. Here, for instance, in at least five languages, is a twelfth century cure for gout:

> Meu, treu, mor, phor,
> Teux, za, zor,
> Phe, lou, chri
> Ge, ze, on.

Perhaps to our forefathers suffering from over-indulgence in the good things of this world, this wondrous group of sounds brought more comfort than the nauseous drugs of the modern practitioner. Any mysterious figure or letter was exceedingly helpful in the sick room of a thousand years ago. The Greek letters "Alpha" and "Omega" had reached England almost as soon as Christianity had, and the old-time doctor triumphantly used them in his pow-wows. Geometric figures in a handful of sand or seeds would prophesy the fate of the ills—and do we not to this day tell our fortune in the geometric figures made by the dregs in our tea-cups? Paternosters, snatches of Latin hymns, bits of early Church ritual were used by quacks of the olden days for much the same reason as the geometric figures—because they were unusual and little understood.

It would have been well had our Anglo-Saxon forefathers confined their healing practises to such gentle homeopathic methods as those mentioned above; but instead desperate remedies were sometimes administered by the determined medicine-man. Diseases were supposed to be caused mainly by demons—probably the ancestors of our present germs—and the physician of Saxon days used all the power of flattery and threat to induce the little monsters to come forth. When the cattle became ill, for instance, the old-time veterinarian shrieked, "Fever, depart; 917,000 angels will pursue you!" If the obstinate cow refused to be cured by such a mild threat, the demons were sometimes whipped out of her, and, if this failed to restore her health, a hole was pierced in her left ear, and her back was struck with a heavy stick until the evil one was compelled to flee through the hole in her ear. Nor was such treatment confined to cattle. The muscular doctors of a thousand years ago claimed they could cure insanity by laying it on lustily with a porpoise-skin whip, or by putting the maniac in a closed room and smoking out the pestering fiends. One did well to retain one's sanity in those good old days.

This use of violent words or deeds in the cure of disease is as ancient almost as the race of man. The early Germans attempted to relieve sprains by reciting confidently how Baldur's horse had been cured by

Woden after all the other mighty inhabitants of Valhalla had given up the task, and even earlier tribes of Europe and Asia had used for illness such a formula as: "The great mill stone that is India's is the bruiser of every worm. With that I mash together the worms as grain with a mill stone." Long after Christianity had reached the Anglo-Saxons of England, the sick often hung around their necks an image of Thor's hammer to frighten away the demon germs that sought to destroy the body. This appeal to a superior being was common to all Indo-European races, and the early Christian missionaries wisely did not attempt to stamp out a belief of such antiquity, but merely substituted the names of Christ, the Virgin Mary and the saints for those of the heathen deities. And even into the nineteenth century this ancient form of faith cure persisted; for there are living yet in Cornwall people who heard, as children, this charm for tooth-ache:

Christ passed by his brother's door,
Saw his brother lying on the floor;
What aileth thee, brother?
  Pain in the teeth.
Thy teeth shall pain thee no more,
In the name of the Father, Son and
Holy Ghost, I command the pain to be gone.

Let us no longer boast of the carefulness of the modern physician; the ceremonies and directions of the Anglo-Saxon doctor were just as painstaking in minuteness and accuracy. When you feel the evil spirits entering you, immediately seek shelter under a linden tree; for out of linden wood were not battle-shields made? Long before Christianity had brought its gentler touches to English life the tribal medicine man wildly brandished such a shield, and sang defiantly to the witch maidens or disease demons:

Loud were they, lo! loud, as over the land they rode;
Fierce of heart were they, as over the hill they rode;
Shield thee now thyself, from their spite thou
  may'st escape thee.
 Out, little spear, if herein thou be!
Underneath the linden stand I, underneath the
  shining shield,
For the might maidens have mustered up their strength,
And have sent their spear screaming through the air!
Back again to them will I send another,
Arrow forth a-flying from the front against them!
 Out, little spear, if herein thou be!

This business of singing was very necessary in the old time doctor's practise. Sometimes he chanted into the patient's left ear, sometimes into his mouth, and sometimes on some particular finger, and the patient evidently had to get well or die to escape the persistent concerts

of his physician. Not infrequently, too, the doctor placed a cross upon the part of one's anatomy to which he was giving the concert, and often the effect was increased by putting other crosses upon the four sides of the house, the fetters and bridles of the patient's horse, and even on the foot prints of the man, or the hoof prints of the beast. Faith in the cross as a charm was unwavering; "the cross of Christ has been hidden and is found," declared the Saxon soothsayer, and by the same token the lost cattle will soon be discovered.

Many and marvelous were the methods to be followed scrupulously by the sick. Cure the stomachache by catching a beetle in both hands and throwing it over the left shoulder with both hands without looking backward. Have you intestinal trouble? Eat mulberries picked with the thumb and ring finger of your left hand. Do you grow old before your time? Drink water drawn silently *down stream* from a brook before daylight. Beware of drawing it upstream; your days will be brief. It reminds one of the practise of the modern herb doctor in peeling the bark of slippery elm *down* if you desire your cold to come down out of your head, or peeling it *up* if you desire the cold to come up out of your chest. One not desiring to place his trust in roots and barks and herbs might turn for aid to the odd numbers, and by reciting an incantation three or seven or nine times might not only regain health, but recover his lost possessions. Or the sufferer might transfer his disease by pressing a bird or small animal to the diseased part and hastily driving the creature away. The ever-willing and convenient family dog might be brought into service on such an occasion by being fed a cake made of barley meal and the sick man's saliva, or by being fastened with a string to a mandrake root, which, when thus pulled from the ground, tore the demon out of the patient.

The cure of children was a comparatively easy task for the Anglo-Saxon doctor; for the only thing to be done was to have the youngster crawl through a hole in a tree, the rim of the hole thus kindly taking to itself all the germs or demons. So, too, minor sores, warts and other blemishes might easily be effaced by stealing some meat, rubbing the spot with it, and burying the meat; as the meat decayed the blemish disappeared. So to this day some Indians, and not a few Mexicans make a waxen image of the diseased part, and place it before the fire to melt as a symbol of the gradual waning of the illness. So, too, the ancient Celts are said to have destroyed the life of an enemy by allowing his waxen image to melt before the fire.

To cure a dangerous disease or the illness of a full-grown man was, however, a much more difficult matter. Inflammation, for instance, was the work of a stubborn demon, and stubborn, therefore, must be the strife with him. Hence, dig around a sorrel plant, sing three paternosters, pull up the plant, sing "Sed libera nos a malo," pound

five slices of the plant with seven pepper corns, chant the psalm "Misere mei, Deus" twelve times, sing "Gloria in excelsis, Deo," recite another paternoster, at daybreak add wine to the plant and pepper corns, face the east at mid-morning, make the sign of the cross, turn from the east to the south to the west, and then drink the mixture. Doubtless by this time the patient had forgotten that he ever possessed inflammation.

Long did the superstitions in medicine persist. In Chaucer's day, the fourteenth century, violent and poisonous drugs were used, but luckily they were often administered to a little dummy which the doctor carried about with him. As we read each day in our newspapers of the various nostrums advertised as curing every mortal ill, we may well wonder if the average credulity has really greatly lessened after twelve centuries of fakes and faith cures, and we almost long for the return of the day when the medicine man practised on a dummy instead of the human body.

EMINENT AMERICAN NAMES

By LAUREN HEWITT ASHE
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THE article entitled "The Racial Origin of Successful Americans," by Dr. Frederick Adams Woods, which appeared in the April (1914) issue of *The Popular Science Monthly,* set forth some very interesting and instructive results. The methods used to arrive at these results, however, do not seem to be such as to establish them as final and conclusive.

It is not sufficient to consider merely the number of persons bearing certain names in "Who's Who in America," for the purpose of establishing the relative capability of various nationalities. The percentage of the number bearing that name in the city in question is the significant figure.

The writer has, therefore, taken the directories[1] of the four American cities, which were the subjects of study in the original article, and has estimated the number of persons of a certain name living in each city by first counting the number of names printed in a whole column of the directory and then multiplying this figure by the number of columns occupied by that name. The number of persons bearing the same name in "Who's Who in America" (1912–1913) is then taken for each city. The percentage is finally calculated of the number of the "Who's Who in America" names in the number of those bearing that name in the directories.

It seems best, furthermore, to narrow down the consideration from the fifty most common names in each city to only those of this number which are common to all four cities in order that any one family may not have too great a weight. The names in each city are then arranged according to the established percentages.

The grouping of names as an indication of race or nationality is taken from Robert E. Matheson's "Surnames in Ireland." It is found to agree exactly with the grouping in the article by Dr. Woods, who classified them from the table given in the New York World Almanac and Encyclopedia for 1914, which table was, no doubt, compiled from Matheson.

[1] (1) Trow's General Directory—Boroughs of Manhattan and Bronx, City of New York, 1913. Trow Directory, Printing & Bookbinding Company, Pub. (2) Boyd's Philadelphia City Directory, 1913. C. E. Howe Company, Pub. (3) The Lakeside Annual Directory of the City of Chicago, 1913. Chicago Directory Company, Pub. (4) The Boston Directory, 1913. Simpson and Murdock Co., Publishers.

NAMES COMMON TO ALL FOUR CITIES, NATIONALITY, ATTRIBUTED TO THEM, AND THE PROPORTION FOR EACH NAME OF THE NUMBER OF TIMES IT OCCURS FOR EACH CITY IN "WHO'S WHO IN AMERICA" (1912–1913) AND THE TOTAL NUMBER OF THE SAME NAME IN THE SAME CITY

New York (Exclusive of Brooklyn)			Chicago			Philadelphia			Boston			Name Averages		
E	White	1.39%	E	Hall	0.72%	E	White	0.46%	E	Allen	0.72%	E	Williams	0.55%
E	Williams	1.18	E–Sc	Moore	0.41	E	Lewis	0.32	E	Williams	0.67	E	White	0.54
E	Clark	1.05	E	Wilson	0.35	E	Taylor	0.31	E	Brown	0.61	E	Taylor	0.44
E	Taylor	1.02	E	Davis	0.27	E	Wilson	0.30	E	Hall	0.43	E	Brown	0.41
E	Jones	0.89	E–Sc	Young	0.27	Sc	Jones	0.27	E	Campbell	0.33	E	Clark	0.40
E	Martin	0.87	E	Thompson	0.25	E–Sn	Johnson	0.23	E	Clark	0.30	E	Wilson	0.37
E	Smith	0.78	E	Brown	0.22	E	Williams	0.22	E	Smith	0.29	E	Jones	0.34
E	Thomspon	0.74	E	Lewis	0.20	E–Sc	Moore	0.20	E	Thompson	0.28	E	Thompson	0.34
E–Sc–G	Miller	0.73	E	Taylor	0.17	E	Davis	0.18	E	Taylor	0.25	E–Sc	Moore	0.34
E	Wilson	0.71	E–Sc–G	Miller	0.17	E–Sc	Young	0.18	Sn–Sc	Anderson	0.22	E	Hall	0.34
E	Brown	0.70	E	Martin	0.16	E	Clark	0.14	E	Lewis	0.20	E	Smith	0.33
E–Sc	Moore	0.60	I	Kelly	0.16	E	Smith	0.13	E–Sn	Johnson	0.19	E	Martin	0.27
E	Davis	0.59	E	Williams	0.15	E	Brown	0.13	E	White	0.18	E	Allen	0.27
E–Sn	Johnson	0.56	E	White	0.14	E–Sc–G	Miller	0.12	E–Sc	Moore	0.17	E	Davis	0.26
Sc–Sn	Anderson	0.55	E	Clark	0.14	E	Martin	0.08	E	Wilson	0.13	E–Sn	Johnson	0.26
I	Murphy	0.46	E	Smith	0.14	E	Thompson	0.08	E	Jones	0.11	E–Sc–G	Miller	0.25
I	Kelly	0.37	E	Allen	0.13	I	Murphy	0.08	I	O'Brien	0.08	E	Lewis	0.21
E	Allen	0.24	Sc	Campbell	0.11	Sc	Campbell	0.08	I	Murphy	0.05	Sn–Sc	Anderson	0.20
E	Hall	0.23	E	Jones	0.10	Sn–Sc	Anderson	0.00	E	Martin	0.00	Sc	Campbell	0.17
Sc	Campbell	0.17	E–Sn	Johnson	0.06	I	Kelly	0.00	E–Sc–G	Miller	0.00	I	Murphy	0.16
I	O'Brien	0.14	I	Murphy	0.06	E	Allen	0.00	E	Davis	0.00	E–Sc	Young	0.14
E	Lewis	0.12	Sn–Sc	Anderson	0.05	E	Hall	0.00	I	Kelly	0.00	I	Kelly	0.13
E–Sc	Young	0.10	I	O'Brien	0.00	I	O'Brien	0.00	E–Sc	Young	0.00	I	O'Brien	0.05

Nationality Averages			Nationality Averages			Nationality Averages			Nationality Averages			Nationality Averages		
G	German	0.73%	E	English	0.22%	E	English	0.18%	E	English	0.25%	E	English	0.34%
E	English	0.69	Sc	Scotch	0.20	Sn	Scandinavian	0.16	Sn	Scandinavian	0.20	G	German	0.25
S	Scandinavian	0.55	G	German	0.17	G	German	0.12	Sc	Scotch	0.14	Sn	Scandinavian	0.24
Sn	Scotch	0.43	I	Irish	0.11	Sc	Scotch	0.11	I	Irish	0.06	Sc	Scotch	0.22
Ic	Irish	0.32	Sn	Scandinavian	0.05	I	Irish	0.02	G	German	0.00	I	Irish	0.12

The nationality attributed to each name is indicated in the tables below by capital letters in the parallel columns. In some cases a name is shared by two or even three nationalities. The percentages belonging to such names are attributed to each of the sharing nationalities in making the final averages. This, of course, is a serious source of error, since the division of such names among the nationalities is not known. No stress can be laid on our figures for the German, Scotch and Scandinavian nationalities, because they contain so many of these indecisive names.

The names in each city are then arranged in groups according to their nationality and averages computed from the percentages established for each name. These averages, which appear at the bottom of each column, give a fair estimation of the capability of the different nationalities, but are, nevertheless, open to a few minor errors. For instance, the Germans head the list in New York with 0.73 per cent. for only one third of a single name, while the English rank second with a total of $15\frac{5}{6}$ names. The final averages for nationality, however, which appear at the bottom of the fifth column and which are made from the averages computed for each city, partly eliminate this error and place the groups in their proper rank.

In order to make the results more conclusive, general averages are drawn for each name from the percentages established for that name in all four cities and are placed in the fifth column according to their rank. Final averages of percentages for nationalities are then made from this column, just as they were for each city. The results obtained agree exactly with the final averages made before and, therefore, are placed coincident with them at the bottom of the fifth column.

The results finally arrived at seem to corroborate the conclusions of Dr. Wood; namely, that in the four leading American cities, New York, Chicago, Philadelphia and Boston, "those of the English (and Scotch) ancestry are distinctly in possession of the leading positions, at least from the standpoint of being widely known." Yet it does not seem safe to disregard entirely those other nationalities which rank so closely with the English merely because of the small number of them included in our consideration; for, as has been stated above, we do not know what proportion of a certain name to attribute to various nationalities.

There is one serious, but unavoidable, source of error, moreover, which has apparently been overlooked. The conclusions as to the relative intelligence of various races are drawn from the number of names, belonging to these races, which appeared in "Who's Who in America." According to the standards of this compilation, eminence is very largely dependent upon education, which does not give the emigrants, who are too poor to get proper education, an equal opportunity to display their

intellectual power and, therefore, to be considered in the above calculations. Races that immigrated predominantly in the last century will be less handicapped than those which have only recently immigrated in large numbers. It is very difficult, however to know how much weight to place upon this modifying influence.

Another source of error is the fact that certain nationalities or races seem to have natural inclinations and desires to follow in disproportionate numbers one kind of activity or occupation and are content to let other people rise to those positions which make them "the best-known men and women of the United States." As Dr. Woods states, the Jews could not be expected to show as large a percentage, since they largely turn their attention to the banking, wholesale and retail trades, in which they have been very successful, but in which eminence is not correspondingly recognized in "Who's Who in America."

No comment is made on Jewish achievement, however, because no Jewish name is among the fifty most common in all four cities, and hence there are not enough numbers for study. But the Irish, by their traditional devotion to politics and their success in attaining the lower ranks of political leadership, would seem to be in line for recognition in large numbers, which they nevertheless do not attain.

In spite of these qualifications, however, it becomes apparent that the statistics above established can not be rejected. Although they do not exactly justify Dr. Woods's conclusions, they at least show that the intellectual achievements of different races vary. They also show that a much more extensive study of the subject must be made before any conclusions can be established as final.

We believe, therefore, that Dr. Woods's conclusion—that "there have been a few notable exceptions, but broadly speaking all our very capable men of the present day have been engendered from the Anglo-Saxon element already here before the beginning of the nineteenth century"—should be modified. A sounder conclusion and, in fact, the only one that could be reached through the results established above, would be this: Achievement in those activities represented in "Who's Who in America" is acquired disproportionately by stocks predominantly Teutonic in comparison with the Irish.

A VISIT TO ŒNINGEN

By Professor T. D. A. COCKERELL
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AS the Rhine broadens on its approach to the Lake of Constance or Boden Sea it flows through a region made classic by the researches of scientific men. Here at low tide it is sometimes possible to see wooden piles which in prehistoric times supported the houses of the lake-dwelling folk, whose work is so well represented in various museums, especially at Zürich. From the river, on each side, the land rises rapidly, and the rounded summits of the hills are well wooded. It is on the left side of the Rhine, about two and a half miles below the town of Stein, that we come to the famous locality for Miocene fossils, the European representative of our Florissant in Colorado.

In all the books the fossil beds are said to be at Œningen, which is the name of a once celebrated Augustinian monastery about two miles away. Actually, however, the locality is above the village of Wangen, which is situated on the north bank of the river. In some quite recent writings Œningen (Wangen) is referred to as being in Switzerland; it is in Baden, though the opposite bank of the Rhine is Swiss. The error is natural, since the fossils have chiefly been made known by the great Swiss paleontologist Heer, of Zürich, and the best general account of them is to be found in his book "The Primæval World of Switzerland," of which an excellent English translation appeared in 1876.

It was at the Œningen quarries, in the eighteenth century, that a wonderful vertebrate fossil, some four feet long, was discovered. A writer of that period, Scheuchzer, announced it as Homo diluvii testis, a man witness of the deluge! Cuvier knew better, and was able to demonstrate its relationship to the giant salamanders of Eastern Asia and North America. It forms, in fact, a distinct genus of Cryptobranchidæ, which Tschudi, apparently mindful of the early error, named *Andrias;* though the proper name of the animal appears to be *Proteocordylus scheuchzeri* (Holl.). The stone at Wangen was used for building purposes, and at one time there were three or four quarries actively worked. In earlier times the larger fossils naturally attracted most attention, fishes, snakes, trutles, fresh-water clams and a variety of leaves and fruits. Such specimens were saved, and were sold and distributed to many museums. The supply was good, yet at times not sufficient for the market; so the monks at Œningen, and others, would carve artificial fossils out of the soft rock, coating them with a brown stain prepared from unripe walnut shells. In later years, during the middle part of the nineteenth century, the period of Darwin, the great importance and interest of the fossil beds came to be better appreciated. Dr. Oswald Heer, professor at Zürich, an accomplished botanist and entom-

ologist, did perhaps nine tenths of the work, describing plants, insects, arachnids and part of the Crustacea. The fishes were described by Agassiz, and later by Winkler. The remaining vertebrates were principally made known by H. von Meyer.

From 1847 to 1853 Heer published in three parts a great work on fossil insects, largely concerned with those from Œningen.[1] In this and later writings he made known 464 species from this locality; but in the latest edition of "The Primæval World of Switzerland" it is stated that there are 844 species, 384 of these being supposedly new, and named, if at all, only in manuscript.

My wife and I, having worked a number of years at Florissant, were very anxious to see the corresponding European locality for fossil insects. The opportunity came in 1909, when we were able to make a short visit to Switzerland after attending the Darwin celebration at Cambridge. We went first to Zürich, where in a large hall in the University or Polytechnicum we saw Heer's collections. A bust of Heer stands in one corner, while one end of the room is covered by a large painting by Professor Holzhalb, representing a scene at Œningen as it may have appeared in Miocene times, showing a lake with abundant vegetation on its shores, and appropriate animals in the foreground. Numerous glass-covered cases contain the magnificent series of fossils, both plants and animals. Dr. Albert Heim, professor of geology and director of the Geological Museum, was most kind in showing us all we wanted to see, and giving advice concerning the precise locality of the fossil beds. Professor Heim is an exceedingly active and able geologist, but neither he nor any one else has continued the work of Heer, whose collections remain apparently as he left them. The 384 supposedly new insects are still undescribed, with a few possible exceptions. I had time only to critically examine the bees, of which I found three ostensibly new forms. Of these, one turned out to be a wasp,[2] one was unrecognizable, but the third was a valid new species, and was published later in *The Entomologist.* There can be no doubt that Heer was too ready to distinguish species of insects in fossils which were so poorly preserved as to be practically worthless, consequently part of those he published and many of those he left unpublished will have to be rejected. Nevertheless, the Œningen materials are extremely valuable, both for the number of species and the good preservation of some of them. All should be carefully reexamined, and the entomologist who will give his time to this work will certainly be rewarded by many interesting discoveries.

Provided with instructions from Professor Heim, we started on August 4 for Wangen, going by way of Constance. Thanks to the map furnished by the Swiss railroad, we had no difficulty in finding the

[1] "Die Insektenfauna der Tertiärgebilde von Œningen und von Radoboj in Croatien" (Leipzig: Engelmann).

[2] *Polistes*, or very closely related to that genus.

Rosegarten Museum in Constance, which contains so many interesting fossils and archeological specimens from the surrounding region. At the moment we arrived, the old man in charge was about to go to lunch, and we were assured that it was impossible to get into the museum. It was then or never for us, however; and when the necessary argument had been presented, the curator not only let us in, but remained with us to point out all the objects of interest, showing a great deal of pride in the collection. The series of Œningen fossils could not, of course, rival that at Zürich; but it contained a great many remarkable things, including some excellent insects. We then boarded the river steamer, and, passing through the Unter Sea, reached the small village of Wangen in the course of the afternoon. This is not a tourist resort of any conse-

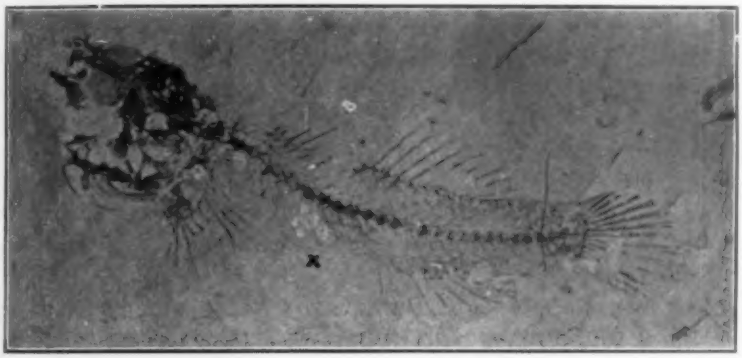


FIG. 1. THE "FISCHLEIN," *Lepidocottus brevis* (Agassiz), showing (at ×) its meal of *Planorbis declivis* A. Br.

quence; the local guide book refers to it as follows: "*Wangen* (with synagogue). Half an hour to the east is the Castle of Marbach, now a well-appointed sanatorium for disorders of the nerves and heart. To the west the romantic citadel Kattenhorn, formerly used as a rendezvous by notorious highwaymen (at present in the possession of a pensioned off German officer)." The guide continues, calling our attention to "*Oberstaad.* Formerly a castle, now a weaving mill for hose. Above it (448 meters) the former celebrated Augustine monastery Oehningen. Near by interesting and curious *stone fossils* are found." Thus the visitor is likely to be misled as to the whereabouts of the fossils, the tradition that they are at Œningen having misled the author of the guide. At Wangen we found a small but most excellent hotel conducted by George Bräuer, where we hastily secured a room, and went out to hunt the fossil beds. We were to walk over half an hour northward, up the hill, and look for the quarries near the top of the high terrace above the village. This we did, but at first without result. We passed a small grassy pit, where some of the rock was visible, but it did not look at all promising. We went back and forth, and up the hill, until we were

practically on the top. The country was beautiful, and by the roadside we found magnificent red slugs (*Arion ater* var. *lamarckii*[3]) and many fine snails, including the so-called Roman snail, *Helix pomatia*. We accosted the peasants, and enquired about the "fossilen." The word seemed to have no meaning for them, so we tried to elucidate it in the manner of the guide: where were the "stein fossilen"? Immediately, with animation, we were shown a road going westward to the town of Stein, where, it was naturally assumed, the object of our enquiry would be found. Quite discouraged, we wandered down the hill until we came to the pit we had noticed when going up. Close by was a neat little cottage, and it occurred to us to try our luck there as a last resort. We were glad indeed when there appeared at the door an educated man, who in excellent Shakespearian English volunteered at once to show us the fossil beds. It was Dr. Ernst Bacmeister, a man of considerable note in his own country, whose life and deeds are duly recorded in "Wer ist's?" He came, with his wife and child, to Wangen in the summer time, to enjoy these exquisite surroundings, where he could write happily on philosophical subjects, without much danger of interruption. Dr. Bacmeister informed us that the poor little pit close by was in fact one of the noted quarries, with the sides fallen in and the debris overgrown with herbage. A short distance away we were shown the others, in the same discouraging condition.

One could see that there had once been considerable excavations, but the good layers were now deeply covered by talus, and could only be ex-

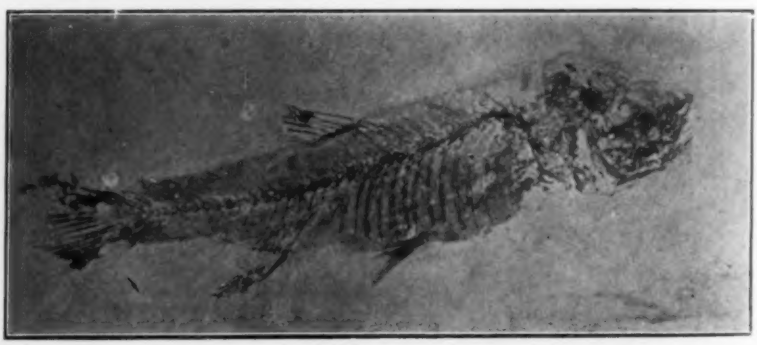


FIG. 2. *Aspius gracilis* Agassiz. Collected at Wangen by Dr. Eugène Penard.

posed after much digging. It was about thirty years since the pits had been worked. Dr. Bacmeister found for us a strong country youth, Max Deschle, who dug under our direction all next day in the quarry near the house. The rock is not so easy to work as that at Florissant, and it does not split so well into slabs, but we readily found a number of fossils.

[3] The earliest name for this richly colored variety is *Limax coccineus* Gistel, but it is not *Limax coccineus* Martyn, 1784; so the next name, *lamarckii*, prevails.

Most numerous were the plants; leaves of cinnamon (*Cinnamomum polymorphum*), soapberry (*Sapindus falcifolius*), maple (*Acer trilobatum*), grass (*Poacites lævis*) and reeds (*Phragmites œningensis*), with twigs of the conifer *Glyptostrobus europæus*. We obtained a single seed of the very characteristic *Podogonium knorrii*. Certain molluscs were abundant; *Planorbis declivis, Lymnæa pachygaster, Pisidium priscum*, with occasional fragments of the mussel *Anodonta lavateri*. Ostracods, *Cypris faba*, were also found. The best find, however, was a well-preserved fish, the *Lepidocottus brevis* (Agassiz), showing in the region of the stomach its last meal, of *Planorbis declivis*. This greatly interested Max, who during the rest of the day chanted, as he swung the pick, "Fischlein, Fischlein, komme!"—but no other Fischlein was apparently within hearing distance. Not a single insect was obtained, except that on the talus at one of the other quarries I picked up a poorly preserved beetle, apparently the *Nitidula melanaria* of Heer.

We left Wangen on the morning of August 6, and proceeded up the Rhine to Schaffhausen and Basle. At Basle we found a certain number of Œningen (Wangen) fossils in the museum.

Comparing Wangen with Florissant, it appears that the Colorado locality is more extensive, more easily worked, and provides many more well-preserved fossils. On the other hand, Wangen has proved far richer in vertebrates and crustacea, and on the whole gives us a better idea of the fauna as it must have existed. Florissant far exceeds Wangen in the number of described species, but this is only because it has so many more insects. Each locality furnishes us with extraordinarily rich materials, enabling us to picture the life of Miocene times. Each, by comparison, throws light on the other, and while the period represented is not sufficiently remote to show much evidence of progressive evolution, it is hard to exaggerate the value of the facts for students of geographical distribution. Much light may also be thrown on the relative stability of specific characters.

Work on the Florissant fauna is going forward, though not so fast as one could wish. It is very much to be hoped that the Wangen quarries will receive attention before many years have passed. Labor is comparatively cheap in Germany, and with a force of a dozen men it would not take long to open up the quarries and get at the best beds. It is really extraordinary that no one has seen and taken advantage of the opportunities presented. Probably no obstacles of any consequence would be put in the way; at least the owner of the quarries came by when we were digging, and expressed only his good will. With new researches in the field, combined with studies of the rich materials awaiting examination at Zürich and elsewhere, no doubt the knowledge we possess of the European Miocene fauna could be very greatly increased, to the advantage of all students of Tertiary life.

THE THEORY AND PRACTISE OF FROST FIGHTING[1]

By ALEXANDER McADIE

ROTCH PROFESSOR OF METEOROLOGY, HARVARD UNIVERSITY

ONLY in recent years have aerologists given much attention to the slow-moving currents of the lower strata of the atmosphere. These differ greatly from the whirls and cataracts of both low and high levels which we familiarly know as the winds. The upper and larger air streams play a part in the formation of frost, and we do not underestimate their function; but primarily it is a slow surface flow, almost a creeping of the air near the ground, which controls the temperature and is all-important in frost formation. So important is it that the first law of frost fighting may be expressed as follows:

Where air is in motion and where there is good circulation, frost is not so likely to occur as where the air is stagnant.

In other words frost in the ordinary meaning of the word is a problem *in local air drainage*. It is true that there are times when with thorough ventilation and mixing of the air strata the temperature will fall rapidly and damage from frost result; but such conditions are perhaps more fittingly described as cold waves or freezes, as distinguished from frosts. Thus, in California during the first week of January, 1913, when there was much air movement, the citrus fruit crop was damaged to the extent of $20,000,000. The condition is generally referred to as a frost, but it was quite different from the usual frost conditions in that section. It is, however, interesting to note that improved frost-fighting devices were used with much success and the total savings aggregated about $25,000,000. The orange growers also had the benefit of accurate forecasts and expert advice and were thus able to provide fuel and labor in advance. Passing over at present the larger disturbances, we shall consider only the frosts of still nights. And it should not be forgotten that the accumulated losses of these frosts may equal the losses of the individual freezes, for the latter occur at long intervals, while the quiet frosts of the early fall and the late spring are recurrent, destroying flowers, fruits and tender vegetation in many sections, year after year.

Air may flow in any direction, but attention has been centered more upon the flow in a horizontal than in a vertical direction. Thus none of the wind instruments used at Weather Bureau stations gives any record of the up and down movement of the air. In frosts of the usual type this vertical displacement is all-important. True, there may be brought into the district, by horizontal displacement, large masses of cold air and the temperature thus materially lowered; but the marked

[1] Some of the instruments used were obtained through a grant from the Elizabeth Thompson Science Fund.

inversion of temperature occurs only when these horizontal currents or winds are lulled. On windy nights, as is well known, there is less likelihood of frost than on quiet nights, because of the thorough mixing of the air vertically. There is then no tendency for stratification and the formation of levels of different temperature, followed by low surface temperature.

In general, the temperature falls as one rises in the air; but, at times of frost, it is found that the higher levels are warmer than the lower

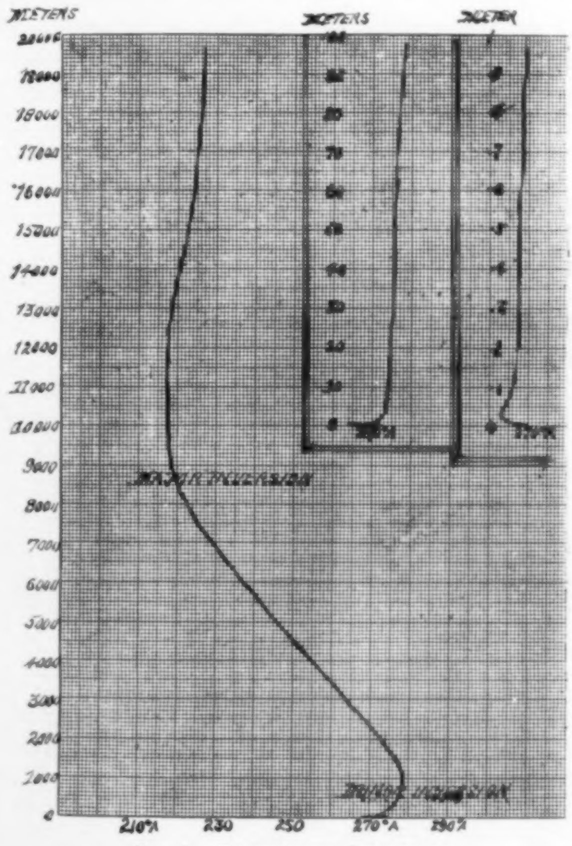


FIG. 1. TEMPERATURE INVERSIONS.

ones. The coldest stratum is found about ten centimeters (four inches) above the ground; while at a distance of ten meters temperatures are as much as five degrees higher than at the ground.

It may be well to refer for a moment to the variations in temperature known as inversions. In the accompanying diagram (Fig. 1) it will be seen that the temperature falls with elevation, and starting from the ground on a day when the temperature is near the freezing point, 273° A.,

one finds at a height of seven thousand meters a fall of about forty degrees. It is not easy to represent on a single diagram the variation in detail and therefore we have divided the air column into three parts, the scales being as one to a hundred.

The right-hand diagram shows the gradual rise in temperature for a height of one meter and the peculiar inversion that occurs a few centimeters above the ground. Unfortunately it is in this layer where detailed temperature observations are most needed that our instruments are least satisfactory. Ordinary thermometers can not be relied on for such small differences and the exploration of this stratum by self-recording instruments is difficult. In the middle diagram is shown the temperature gradient at times of frost, from the ground to a height of one hundred meters. It will be seen that at a height of fifty meters the temperature may be ten degrees higher; and in general the rise continues with elevation. A good illustration of a valley inversion is given by the chart of May 20 (Fig. 2), in which continuous records for

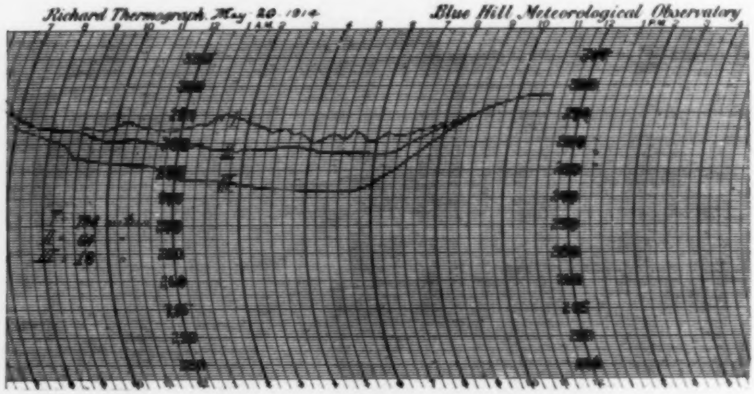


FIG. 2. UPLANDS WARM, LOWLANDS COLD.

three levels, 18, 64 and 196 meters above sea level, are given. At such times fruit or flowers on hillsides escape damage from frost while in all the depressions and low level places the injury may be marked. These differences in temperature are not at all unusual and may be anticipated on clear, still nights during spring, fall and winter. Clouds or a moderate wind will prevent such an inversion. We shall refer again to this in speaking of the cranberry bogs of the Cape Cod district and the frost warnings issued from Blue Hill Observatory.

The great inversion in the atmosphere, however, is that which we have indicated as occurring at the height of nine thousand meters. Above this, the temperature ceases to fall and we enter what has been called the stratosphere or isothermal region. For convenience we will call this upper change the *major* inversion and the lower one near the

ground the *minor* inversion. In some ways we know more about the former than the latter. Strictly speaking, the minor inversion is the chief factor in determining local climate since it controls night and early morning temperatures and in large measure the early or late blooming of flowers and ripening of fruits.

Ordinarily cold air falls to the ground; but not always, for under certain conditions cold, heavy air may actually rise, displacing warm, lighter air. But such conditions can be explained and there is no contradiction of the fundamental law that if acted on only by gravity, cold air, being denser, will settle to the ground and warm air, being lighter, will rise. And there must be a certain relation between the height of the level from which the cold air falls and the level to which the warm air rises. In other words, we have to apply the laws of falling bodies since a given mass of air, although invisible, is matter and as subject to gravity as a cannon ball.

One of Galileo's most ingenious experiments consisted in swinging a pendulum and then by means of a nail driven in various positions intercepting the swing. He found that the bob always rose to the same level whatever circuit it was forced to take. But Galileo did not know what every schoolboy to-day knows, that air exerts pressure and is subject to physical processes like other matter, else he would certainly have given to the world a delicate air pendulum; and devised experiments on the movement of air that would have opened men's eyes to the fascinating flow and counter-flow of the air, even on a seemingly still night, one favorable for the formation of frost.

The problem of the moving air mass, however, is more complicated than it looks. For with the air is mixed a quantity of water vapor. In a strict sense they are independent variables, and the view set forth in most text-books that air has a certain capacity for water vapor is misleading. We seldom meet with pure, dry air. A cubic meter of such a gas mixture would weigh 1,247 grams, at a temperature of 283° A. (50° F.). If chilled ten degrees, that is, to the freezing point of water, it would weigh 46 grams more. So that by cooling, air becomes denser and heavier. A cubic meter of a mixture of air and water vapor at saturation, at the first temperature above mentioned weighs only 1,242 grams, or five grams less, and if this were cooled ten degrees the mixture would weigh three grams less than the same volume of pure dry air. We see that in each case the mixture of air and water vapor weighs less than the air by itself. One would think that by adding water vapor which, while light, still has weight, the total weight would be the sum of both. It really is so, notwithstanding the above figures, and the explanation of the puzzle is that there was an increase in pressure with expansion, so that the volume of the air and saturated vapor was greater than one cubic meter. Since then a cubic meter of air and saturated vapor weighs less

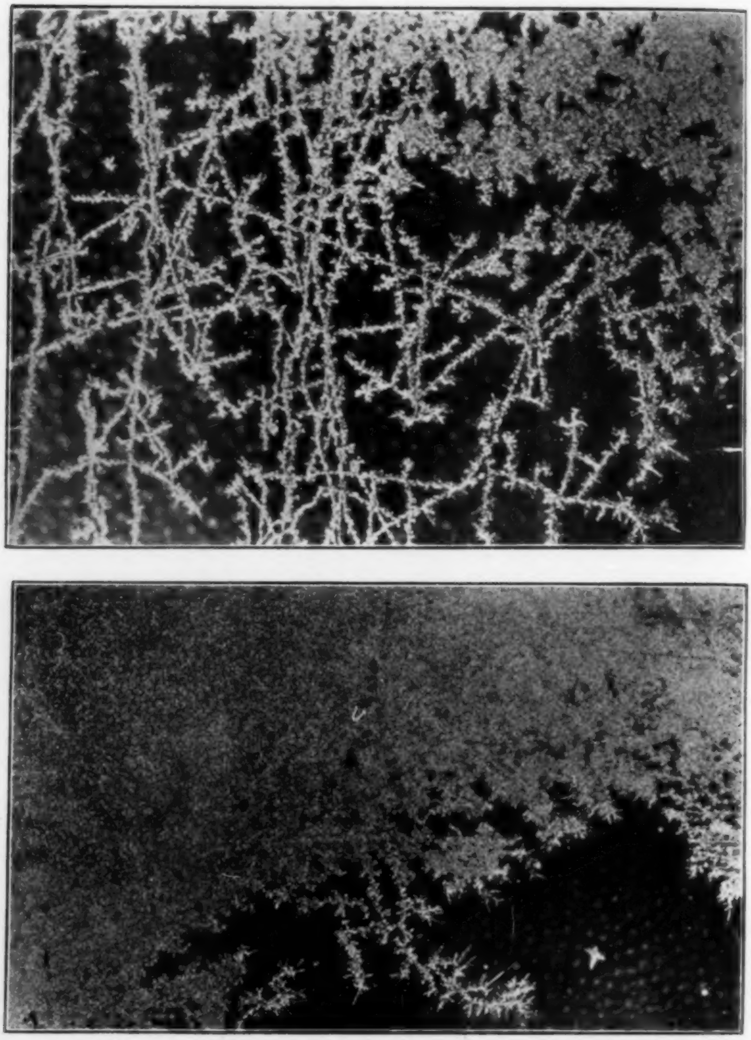


FIG. 3. FROST ON GLASS.

than a cubic meter of dry air at freezing temperature, speaking generally, we may expect moist air to rise and dry air to fall. Consequently, if in addition to falling temperature there is also a drying of the air, we shall have an accelerated settling or falling of cold dry air to the ground, which of course favors the formation of frost. The water vapor plays also another rôle besides that of varying the weight per unit volume. The heat received by the ground consists of waves of a certain wave-length; but the heat re-radiated by the ground consists of waves of longer wave-length, and these so-called long waves (12 thousandths of a

millimeter) are readily absorbed by water vapor. Thus water vapor acts like a blanket and holds the heat, preventing loss of heat by radiation to space. Further on we shall speak of the high specific heat of both water and water vapor as compared with air and show the bearing of this in frost fighting; but at present we may from what precedes formulate the second law of frost fighting as follows: "Frost is more likely to occur where the air is dry than where it is moist." It is also true that a dusty atmosphere is less favorable for frost than a dust-free atmosphere. Thus we may generalize and say that whatever favors clear, still, dry air favors frost. The theory of successful frost fighting then is to interfere with or prevent these processes which as we have seen facilitate cooling close to the ground. In what way can this best be done?

The most natural way would be by conserving the earth's heat, which could be accomplished by covering plants with cloth, straw, newspaper, or perhaps better still, modern weather-proof sheeting, or in still another way by a cover of moistened dense smoke, generally called a smudge. A second method would be by means of direct application of heat; and this is accomplished in orange groves by means of improved orchard heaters. Large fires waste heat and are neither economical nor effective. A third method would be based upon a mixing of the air strata, thus getting the benefit of the warmer higher levels. Fourth, advantage might be taken of some agency such as water or water vapor, having a high specific heat. Finally, if the crop is of a certain character such as the cranberry, it will be found advisable to use sand, to drain and clean, here again making use of the specific heat of some intermediary. And, furthermore, any one of these methods may be combined with some other method.

Regarding the first method, that of covers, it may be said that the practise goes back to the early husbandmen; but only in the last few years has the true function of the cover been properly interpreted and we are still far from obtaining maximum efficiency. Nor is there yet a suitable, scientific cover available. Any medium that interferes with

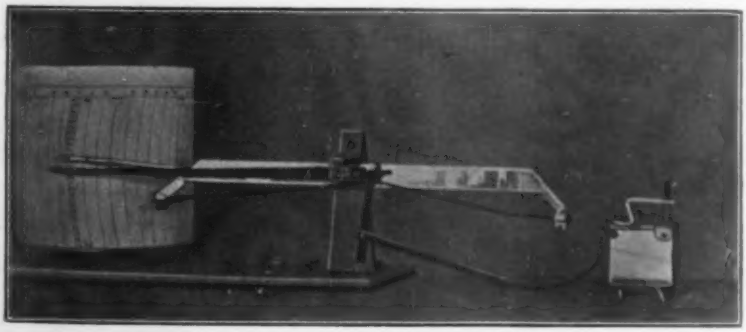


Fig. 4. Saturation Deficit Recorder. McAdie.

loss of heat through free radiation before and after sunset is a cover. The best type of cover is a cloud; and clouds, whether high or low, are good frost protectors. On cloudy nights there is little likelihood of frost; and when we can bring about the formation of a layer of condensed water vapor we can practically eliminate frost. We have mentioned above the fact that the earth radiates the heat it has received not in the same but in longer wave-lengths perhaps three times as long. These are easily trapped and held by the vapor of water. Furthermore, the rate of radiation is a function of the absolute temperature and so the rapidity of loss depends somewhat upon the heat received. Therefore the cover should be used as early in the afternoon as possible, that is just before sunset. Aside from the water cover or vapor cover there are cheap cloth screens, fiber screens and in some places lath screens.

The second method, that of direct heating, has met with much success in the orange groves of California and elsewhere. Modern heating and

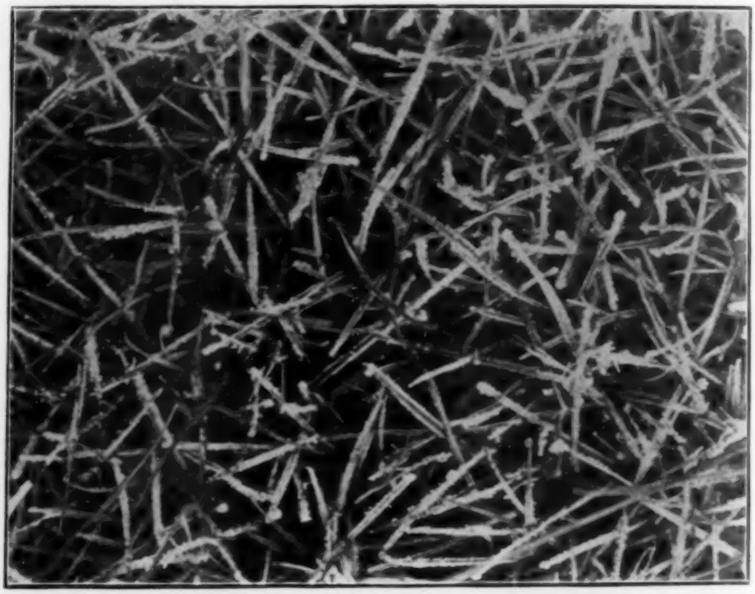


FIG. 5. FROST ON GRASS. Photo. by O. H. Parker.

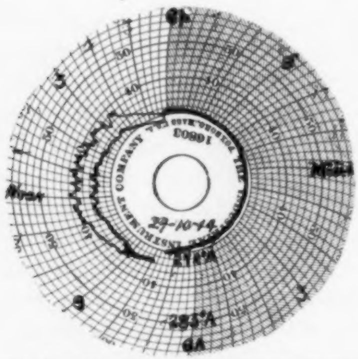
covering methods date from experiments begun in 1895. A number of basic patents granted to the writer in this connection have been dedicated to the public. At the present time there are on the market some twenty forms of heaters, which have been described with more or less detail in farm journals and official publications. It is not necessary to refer to them further here. The fuel originally used was wood, straw and coal, but these are now supplanted by crude oid or distillate. It has also been seriously proposed to use electric heaters; also to use gas in the groves.

With modern orchard heaters properly installed and handled, there is no difficulty in raising the temperature of even comparatively large tracts five degrees and maintaining a temperature above freezing, thus preventing refrigeration of plant tissue.

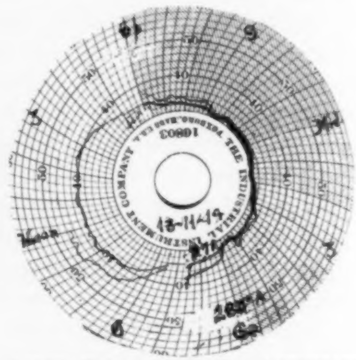
The third method, that of utilizing the heat of higher levels by mixing, has not yet been commercially developed; but the methods of applying water, either in the spraying of trees or the running of ditches or the flooding of bogs, together with methods of sanding, cleaning and draining, have all been proved helpful. Methods available and most effective in one section may not necessarily be effective in another section or with different crop requirements. Certain devices most effective in the groves of California may not answer in Florida or Louisiana because of entirely different weather conditions. In the Gulf coast states where water is available it may be advantageously used to hold back ripening and retard development until after the cold waves of middle and late February have passed, whereas in the west coast sections conditions are very different, water having a definite value and the critical periods coming in late December or early January.

In what precedes stress has been laid chiefly upon the fall of temperature and the congelation of the water vapor. There is, however, another important matter connected with injury to plant tissue, and that is the rise in temperature *after* the frost. A too rapid defrosting may do considerable damage where no damage was originally done by the low temperature. It is in this connection that water may be used to great advantage. Water, water-vapor and ice have, compared with other substances, remarkably high specific heats. If the specific heat under constant pressure of water be taken as unity, that of ice is 0.49; of water-vapor 0.45 and of air 0.24. Or in a general way we may say that water has four times the capacity for heat that air has. Therefore it is apparent that water will serve excellently to prevent rapid change in temperature. This is important at sunrise and shortly after when some portion of the chilled plant tissue may be exposed to a warming sufficient to raise the temperature of the exposed portion ten degrees in an hour. The latent heat of fusion of ice is 79.6 calories and the latent heat of vaporization of water is nearly 600 calories (a gram calorie is the amount of heat that will raise the temperature of a gram of pure water one degree) or in exact terms from 273° A. to 274° A. Therefore in the process of changing from solid to liquid to vapor, as from ice to water to vapor, there is a large amount of heat required. The latent heat serves to prevent fall in temperature and also serves to retard a too rapid rise. This does not mean, as is generally assumed, that the air will be warmed, but it does mean a retardation of temperature change. And it is essential that the restoration of the tissues and juices to their normal state be accomplished gradually, neither too rapidly nor yet too slowly.

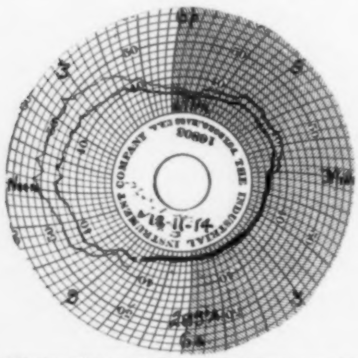
There is probably an optimum temperature for thawing or defrosting frozen fruits and flowers. Finally the temperature records as ordinarily obtained need careful interpretation. It may be that the freezing point



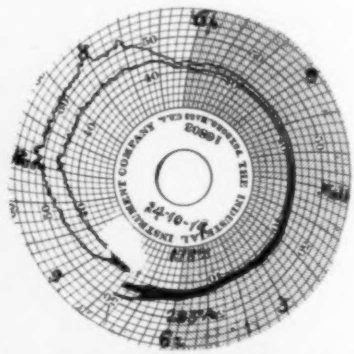
HEAVY FROST, CORRECTLY ANTICIPATED.



LIGHT FROST, CORRECTLY ANTICIPATED.



HEAVY FROST CORRECTLY INDICATED BUT WARMER AFTER 3 A.M.



LIGHT FROST INDICATED BUT INCORRECTLY. CLOUDY WEATHER AFTER 6 P.M.

of liquids under pressure in the plant cells or exposed to the air through the stomata is not the same as in the free air. It is unfortunate too that in most places data showing temperatures of soil, plant and air are of doubtful character. A word of warning may be given against the too ready acceptance of Weather Bureau records made in cities and on the roofs of buildings. Garden and field conditions vary greatly from these. It is further advisable to obtain a continuous record of the temperature of evaporation such as is shown by the records herewith. The two temperature curves made simultaneously and easily read at any moment enable the gardener or orchardist to forecast the probable minimum temperature of the ensuing ten or twelve hours. But not always, and some study is necessary. A slight increase in cloudiness or a slight shift in wind direction will prevent the fall in temperature which otherwise

seemed probable. With a persistent inversion of temperature there is sometimes an increasing absolute humidity.

Summary

The problem is many sided and we must consider the motion of the air vertically as well as horizontally. Air gains and loses heat chiefly by convection, and any gain or loss by conduction may be neglected. The plant gains heat by convection, radiation and perhaps by conduction of an internal rather than surface character. The ground gains and loses heat chiefly by radiation. But the whole process is complicated and may not even be uniform. Frosts generally are preceded by a loss of heat from the lower air strata, due to convection and a horizontal translation of the air. Then follows an equally rapid and great loss of heat by free radiation. There are minor changes such as the setting free of heat in condensation and the utilization in evaporation, but these latent heats are of less importance than the actual transference of the air and vapor and the removal of the latter as an absorber and retainer of heat.

Frosts are recurrent phenomena reasonably certain to occur within given dates, and, as pointed out above, the cumulative losses are considerable. Methods of protection to be serviceable must be available for more than one occasion, for there is no profit in saving a crop on one night and losing it on the succeeding night. But the effort is worth while. Consider that the horticulturist regularly risks the labor of many months on the temperatures of a few hours. An efficient frost fighting device is in a way the entering wedge for solving problems of climate control. One may not take a crop indoors, it is true, but there is no valid reason, in the light of what has been already accomplished, why at critical periods which may be anticipated, the needed volume of surface air may not be sufficiently warmed; and the losses which have heretofore been considered inevitable be prevented.

The American Museum of Natural History,
In which the autumn meeting of the National Academy of Sciences was held.

THE PROGRESS OF SCIENCE

THE NEW YORK MEETING OF THE NATIONAL ACADEMY OF SCIENCES

The National Academy of Sciences held its annual autumn meeting during the third week of November in the American Museum of Natural History. The central situation of New York City and its scientific attractions led to a large meeting and an excellent program. There were present over sixty members, nearly one half of a membership widely scattered over the country. When the academy was established in 1863 as the adviser of the government in scientific questions, the membership was limited to fifty which was subsequently increased to 100, under which it was kept until recently. The present distribution of the 141 members among different institutions in which there are more than two is: Harvard, 19; Yale, 15; Chicago, 13; Johns Hopkins, 12; Columbia, 11; U. S. Geological Survey, 8; Carnegie Institution, 5; California, Rockefeller Institute, Smithsonian, 4; Clark, Wisconsin, Cornell, Stanford, 3.

The scientific program of the meeting began with a lecture by Professor Michael I. Pupin, of Columbia University, who described the work on aerial transmission of speech of which no authentic account has hitherto been made public. To Professor Pupin we owe the discovery through mathematical analysis and experimental work of the telephone relays which recently made speech by wire between New York City and San Francisco possible, and we now have an authoritative account of speaking across the land and sea a quarter way round the earth. One session of the academy was devoted to four papers of general interest. Professor Herbert S. Jennings, of the Johns Hopkins University, described experiments showing evolution in progress, and Professor John M. Coulter, of the University of Chicago, discussed the causes of evolution in plants. Professor B. B. Boltwood made a report on the life of radium which may be regarded as a study of inorganic evolution. Professor Theodore Richards, of Harvard University, spoke of the investigations recently conducted in the Wolcott Gibbs Memorial Laboratory. These are in continuation of the work accomplished by Professor Richards in the determination of atomic weights, which led to the award to him of a Nobel prize, the third to be given for scientific work done in this country, the two previous awards having been to Professor Michelson, of the University of Chicago, in physics, and Dr. Carrel, of the Rockefeller Institute, in physiology.

Of more special papers, some of which, however, were of general and even popular interest, there were on the program 36, distributed somewhat unequally among the sections into which the academy is divided as follows: Mathematics, 0; Astronomy, 3; Physics and Engineering, 7; Chemistry, 1; Geology and Paleontology, 6; Botany, 7; Zoology and Animal Morphology, 8; Physiology and Pathology, 4; Anthropology and Psychology, 0. A program covering all the sciences belongs in a sense to the eighteenth rather than to the twentieth century; still there is human as well as scientific interest in listening to those who are leaders in the conduct of scientific work.

The academy was fortunate in meeting in the American Museum of Natural History, where in addition to the scientific sessions luncheon and an evening reception were provided. The museum has assumed leadership both in exhibits for the public and in the scientific research which it is accom-

Members of the National Academy of Sciences at the Entrance of the American Museum of Natural History.

ENTRANCE TO THE AMERICAN MUSEUM OF NATURAL HISTORY.

plishing. The planning of museum exhibits is itself a kind of research and in this direction the American Museum, together with the National Museum in Washington and the Field Museum in Chicago, now surpasses any of the museums of the old world and in the course of the next ten years will have no rivals there. It is interesting that the city and an incorporated board of trustees are able to cooperate in the support of the museum, as is also the case with the Zoological Park and the Botanical Gardens which the members of the academy visited in the course of the meeting.

FREDERIC WARD PUTNAM

POWELL in Washington, Brinton in Philadelphia and Putnam in Cambridge may be regarded as the founders of modern anthropology in America. In the death of Putnam, at the age of seventy-six years, we have lost the last of these leaders.

Putnam is often spoken of as the father of anthropological museums because he, more than any other one person, contributed to their development. He seems to have been a museum man by birth, for at an early age we find him listed as curator of ornithology in the Essex Institute of Salem, Mass. The Peabody Museum of Archeology at Cambridge is largely his work, he having entered the institution in 1875 and continued as its head until his death. This institution is in many respects one of the most typical anthropological museums in America. Dur-

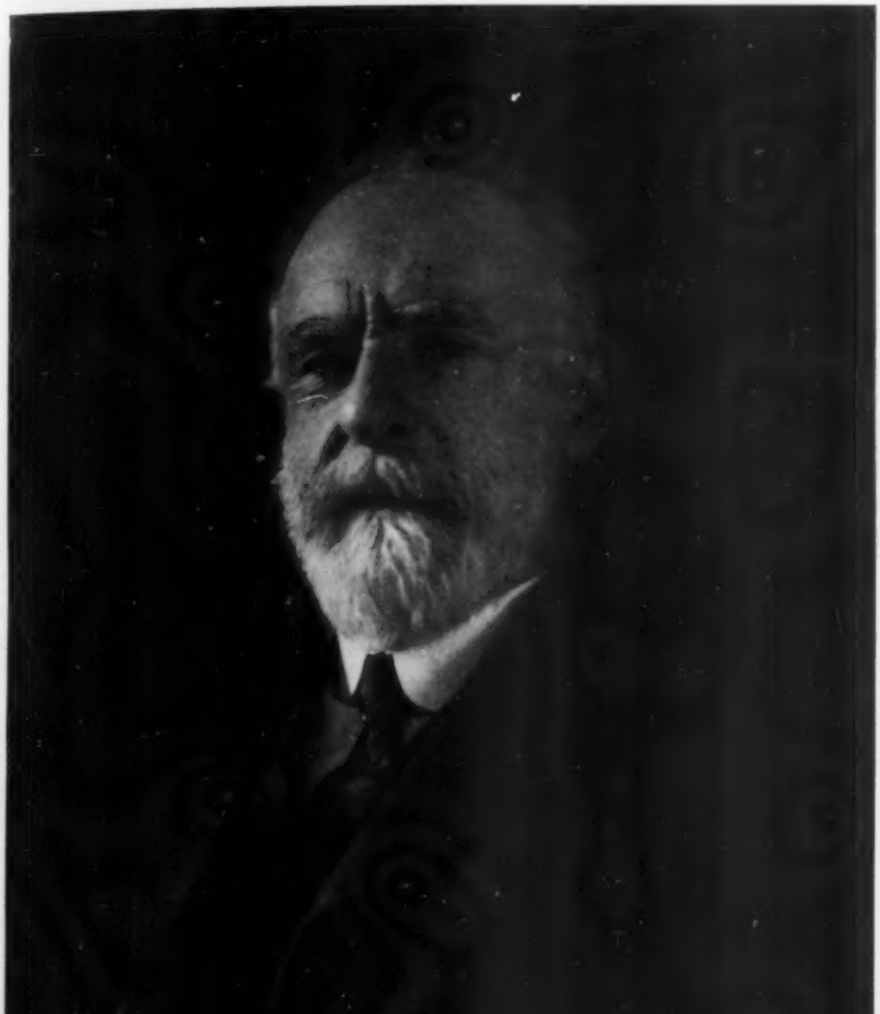
ing his college career Professor Putnam came under the influence of Professor Louis Agassiz and was for several years an assistant in the laboratory of that distinguished scientist. It seems likely that this was the source of Professor Putnam's faith and enthusiasm for the accumulation and preservation of concrete data. As his interest in anthropology grew, he seems to have sought to bring together in the Peabody Museum a collection of scientific material that should have the same relation to the new and developing science of anthropology as the collections of Professor Agassiz's laboratory had to the science of biology. Professor Putnam's great skill in developing the Peabody Museum brought him into public notice and led to his appointment as director of the anthropological section of the World Columbian Exposition in Chicago. The exhibit he prepared made an unusual impression and it is said that largely to his personal influence is due the interest of the late Marshall Field in developing and providing for the museum which now bears his name. After this achievement Professor Putnam was invited by the American Museum of Natural History to organize the department of anthropology which he proceeded to do upon broad lines, giving it a status and impetus which is still manifest. Later on he was invited to the University of California to organize a department and a museum similar to the one at Harvard and this also is now one of our leading institutions. Thus it is clear that the history of American anthropological museums is to a large extent the life history of Professor Putnam.

The one new and important idea which Professor Putnam brought into his museum work was that they should be in reality institutions of research. Until that time they were chiefly collections of curios brought together by purchase of miscellaneous collections without regard to the scientific problems involved. Professor Putnam's idea was that the museum should go into the field and by systematic research and investigation develop a definite problem, bringing to the museum such illustrative and concrete data as should come to hand in the prosecution of research. Professor Putnam also played a large part in securing the recognition of anthropology by universities and by his position at Harvard pointed the way to mutual cooperation between museums and universities. He possessed an unusual personality which enabled him to approach and interest men of affairs so as to secure their financial support for anthropological research and as a teacher he was intensely interested in young men, offering them every possible opportunity for advancement and never really losing personal interest in them as long as he lived.

SCIENTIFIC ITEMS

WE record with regret the deaths of Brigadier-general George M. Sternberg, retired, surgeon-general of the army, from 1893 to 1902, distinguished for his investigations of yellow fever and other diseases; of Edward Lee Greene, associate in botany at the Smithsonian Institution; of Wirt Tassin, formerly chief chemist and assistant curator of the division of mineralogy, U. S. National Museum; of Augustus Jay Du Bois, for thirty years professor of civil engineering in the Sheffield Scientific School, Yale University; of Sir Andrew Noble, F.R.S., distinguished for his scientific work on artillery and explosives; of Edward A. Minchin, F.R.S., professor of protozoology in the University of London, and of R. Assheton, F.R.S., university lecturer in animal embryology at the University of Cambridge.

THE Nobel prize for chemistry for 1914 has been awarded to Professor Theodore William Richards, of Harvard University, for his work on atomic weights. The prize for physics has been awarded to Professor Max



Frederic Ward Putnam.

von Laue, of Frankfort-on-Main, for his work on the diffraction of rays in crystals.

PROFESSOR ADOLF VON BAEYER celebrated his eightieth birthday on October 31. With the beginning of the present semester he retired from the chair of chemistry at Munich in which he succeeded von Liebig in 1875.—The Romanes lecture before the University of Oxford will be delivered this year by Professor E. B. Poulton, Hope professor of zoology in the university, on December 7. The subject will be "Science and the Great War."

AT the recent meeting in Manchester, as we learn from *Nature,* the general committee of the British Association unanimously adopted the following resolution, which has been forwarded to the Prime Minister, the Chancellor of the Exchequer and the Presidents of the Board of Education and of Agriculture and Fisheries: "That the British Association for the Advancement of Science, believing that the higher education of the nation is of supreme importance in the present crisis of our history, trusts that his Majesty's government will, by continuing its financial support, maintain the efficiency of teaching and research in the universities and university colleges of the United Kingdom."

COLUMBIA UNIVERSITY received by the will of Amos F. Eno the residuary estate which may amount to several million dollars. In addition, the General Society of Mechanics and Tradesmen receives $1,800,000, and bequests of $250,000 each are made to New York University, The American Museum of Natural History, the Metropolitan Museum of Art and the New York Association for improving the Condition of the Poor.—Mr. James J. Hill has presented $125,000 to Harvard University to be added to the principal of the professorship in the Harvard graduate school of business administration, which bears his name. The James J. Hill professorship of transportation was founded by a gift of $125,000, announced last commencement day, the donors including John Pierpont Morgan, Thomas W. Lamont, Robert Bacon and Howard Elliott.—The sum of about $400,000 has been subscribed in the University of Michigan alumni campaign for $1,000,000 with which to build and endow a home for the Michigan Union, as a memorial to Dr. James B. Angell, president emeritus.

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The Scientific Monthly, October, November, December

NAMES OF CONTRIBUTORS ARE PRINTED IN SMALL CAPITALS

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EDITED BY J. McKEEN CATTELL

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RECENT PUBLICATIONS

Locy's Biology and Its Makers.

New Editio

By WILLIAM A. LOCY, Professor in Northwestern University. xxvi + 477 pp 8vo. *Illustrated.* $2.75 net.

In this third revised edition, several alterations have been made, though th original form is substantially retained. Several pages have been rewritten to convey more clearly the meaning, as in reference to Mendel's discovery, and some addition have been made, as comments on isolation and orthogenesis as factors of organic evolu tion. The important contributions of Fritz Schaudinn have been noted, and the dis cussion of the antiquity of man has been considerably extended. Several new portrait have been substituted for those of the earlier editions.

In a review of *Die Biologie und Ihre Schöpfer*, the authorized German translation of this book, the June 1915 number of *Zoologische Annalen* says:

"Die Hauptsache bleibt, dass eine nicht zu umfangreiche und doch Wesentliche bringende geschichtliche Darstellung der Biologie vorliegt, an der es bisher gefehlt hat sie ist flüssig geschrieben, liest sich leicht und ist wohl geignet, Interesse zu erwecken und zu fördern. Wir möchten das Buch in der Hand jedes sehen, der sich mit biolog ischen Wissenschaften beschäftigt, der Studierenden sowohl wie der bereits im Berufe Stehenden und auch sonstigen Interessierten; sie alle werden es mit hoher Befriedigung benützen und reichen Gewinn davontragen."

Townsend's Functions of a Complex Variable.

By E. J. TOWNSEND, Professor and Head of the Department of Mathematics in the University of Illinois. (*American Mathematical Series.*) vii + 384 pp. 8vo. $4.00.

This book is based upon a course of lectures given by the author for a number of years at the University of Illinois. It is intended as an introduction to the subject, suitable for seniors and first year graduate students, and can be completed in a three or four hour course of a year. It presupposes a knowledge of calculus such as is usually given in the undergraduate course. Such topics from real variables as are not ordinarily included in an elementary course in calculus, and made use of in the development of the theory of functions of a complex variable are discussed in the text.

Both the Cauchy-Riemann and the Weierstrass points of view are made use of in developing the general properties of analytic functions. In a chapter following the discussion of the general properties of single-valued functions, the author shows how those properties may be extended to the consideration of multiple-valued functions by the aid of Riemann surfaces.

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THE SCIENTIFIC MONTHLY AND THE POPULAR SCIENCE MONTHLY

The Popular Science Monthly, since its establishment in 1872 by E. L. Youmans and the firm of D. Appleton and Company, has endeavored to perform two functions which are somewhat distinct. On the one hand, it has aimed to popularize science, and, on the other hand, to publish articles reviewing scientific progress and advocating scientific, educational and social reforms. The objects are both important, but as science grows in complexity it becomes increasingly difficult to unite them in the same journal.

In the earlier years of The Popular Science Monthly the doctrine of evolution excited controversy and wide public interest; it was possible to print articles by men such as Darwin, Spencer, Huxley and Tyndall, which were popular and at the same time authoritative contributions to scientific progress. Dr. Youmans had the fervent faith and missionary spirit which enabled him to conduct a journal to which the word "popular" was properly applied. At that time other magazines, such as *The Atlantic* and *Scribner's*, also published articles and had departments concerned with popular science.

The last third of the nineteenth century may properly be characterized as the era of science, so rapid was the progress of science and so important the part it assumed in our civilization. This progress not only requires specialization of work, but even makes it difficult for the worker in one field to understand the work accomplished in other fields, though the barrier is perhaps due to terminology rather than to ideas. For the general public the difficulties are greater, and there is danger lest it may lose touch with the advances of science. But in a democracy in which science must depend on the people for support and for recruits, it is essential that a sympathetic understanding be maintained. For this purpose two journals are needed rather than one, for it is necessary to address those having different interests.

During the fifteen years since 1900, the editor of The Popular Science Monthly aimed to conduct a journal maintaining high scientific standards and discussing authoritatively problems of scientific importance. The journal was popular, in the sense that it was not special or technical and could be understood by those having education and intelligence, but it was not popular in the sense that it appealed to all people and might number its subscribers by the hundreds of thousands. Manuscripts were received in large numbers which were clearly intended for a magazine of different type, and such a magazine is needed. A well-illustrated magazine devoted to the popularization of science should have a wide circulation and be conducted on different lines from a journal concerned with the less elementary aspects of scientific work, just as a high school and the graduate school of a university differ in their methods and in their appeal.

A group of men desiring a journal to which the name The Popular Science Monthly will exactly apply, this publication has been transferred to them, while, beginning in October, a journal on the present lines of The Popular Science Monthly will be conducted under the more fitting name of The Scientific Monthly. This differentiation of The Popular Science Monthly into two journals is in the natural course of evolution, each journal being able to adapt itself to its environment more advantageously than is possible for a single journal. Each can perform an important service for the diffusion and advancement of science.—From an editorial statement in The Popular Science Monthly for September, 1915.